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ART. VIII.—*On the Photometric comparison of Light of different colors;* by Professor O. N. ROOD, of Columbia College.

[Read before the National Academy of Sciences, Oct. 24th, 1877.]

THE comparison of the intensities of light of different colors has long been considered one of the most difficult of photometric problems, but by the use of very simple means, I have recently made a series of measurements of this character, which may not be without interest to those whose studies lie in this direction. The luminosity of card-board painted with vermilion, was, for example, measured as follows: a circular disc of the vermilion card-board was attached to the axis of a rotation-apparatus, smaller circular discs of black and white card-board being simultaneously fastened on the same axis, so that by varying the relative proportions of the latter, a series of grays could be produced at will.

The compound black and white disc was now arranged so as to furnish by rotation a gray which was *decidedly darker* than the vermilion; this gray tint was then gradually lightened, till the observer became doubtful as to the relative luminosities of the red and gray discs; the angle occupied by the white sector was then measured. Next, a gray decidedly *more* luminous than the vermilion was compared with it, and diminished in brightness till the observer again became doubtful, when a second measurement was taken. All this time the manipulation was performed by an assistant, the experimenter giving directions, but remaining in ignorance of the results to the end. The mean of ten such experiments assigned to the vermilion disc a luminosity of 23·8, that of white card-board being taken

as 100. In this experiment and in all those that follow, proper corrections were made for the amount of white light reflected by the black disc, this having been previously ascertained, in a manner which will be described in a future communication.

In order to test the correctness of the final result, the luminosity of a blue-green disc, correctly complementary in color to the vermilion, was next measured in the same way; it proved to be 26·56. The vermilion and blue-green discs were then combined, according to Maxwell's method, so as to obtain a pure gray by rotation, and the angular proportions of these colored surfaces, and the value of the gray in terms of white and black card-board, measured. The gray thus obtained had a luminosity of 24·54, that of white card-board being 100. Next, the value of this same gray was *calculated* from the measured luminosities of the two colored discs, and the proportions of these colors required to produce a pure gray by mixture on the rotation apparatus; the calculated value was 25·47.

This agreement proves the correctness of the photometric comparison, and also of Grassman's assumption, that the total intensity of the mixture of masses of differently colored light is equal to the sum of the intensities of the separate components, which, so far as I know, has not before received an experimental confirmation.

Corresponding measurements were made with a green, and its complementary purple disc; also with a blue, and its complementary yellow disc; the results are given below.

	Luminosity.	Gray (observed.)	Gray (calculated.)
Vermilion,	23·8	24·54	25·47
Blue-green,	26·56		
Chrome yellow,	80·3	54·51	53·92
Cobalt blue,	35·38		
Green,	41·19	24·94	26·56
Purple,	14·83		

ART. IX.—*The Geological Commission of Brazil. Additions to the Echinoid fauna of Brazil;* by RICHARD RATHBUN.

THOUGH the Geological Commission has busied itself in investigating the marine fauna at almost every point on the coast of Brazil where its labors have extended, yet in a group like that of the *Echini*, the different shore species of which are so readily found and collected by all travelers, it has been able to add very little that is new. In its collections, however, are three species which do not seem to have been recorded from Brazil before. They are: *Diadema setosum* Gray, *Hipponoë*

esculenta A. Agassiz, and *Mellita sexforis* A. Agassiz. According to Mr. Alex. Agassiz in his "Revision of the Echini," the first mentioned species is almost world-wide in its distribution, occurring in the seas between North and South America and Mexico, among the West Indies, the islands off the northwest of Africa, and also in many places in the Pacific and Indian Oceans. We cannot yet record it from the mainland of Brazil, but it was found in great abundance by Mr. J. C. Branner, on the shores of the island of Fernando de Noronha, situated nearly 200 miles to the northeast of Cape St. Roque.

The second species, *Hipponoë esculenta*, was obtained by Mr. Branner from the same locality and also from the mouth of Rio Formoso, about twenty miles south of Cape St. Agostinho, in the province of Pernambuco. In its previously known range, as recorded by Mr. Agassiz, it was limited to the main shores and islands of the seas between North and South America, and to the Bermudas.

The third and last species mentioned, *Mellita sexforis*, was dredged from the shallow water inside of Fort Villegagnon, in the harbor of Rio de Janeiro, by Professor E. Selenka, who has kindly presented the Commission with several specimens. They are very numerous in this one locality, but, so far as we know, have not been obtained from any other part of Brazil. Of this species, Mr. Alex. Agassiz says that it has a great range in depth, from shallow water to 270 fathoms, but has not been found over an extended geographical area, as it has been recorded from the southern United States, the West Indies and Bermudas, only.

In addition to the two species above mentioned from Fernando de Noronha, there were obtained from the same locality three other species already known from the coast of Brazil: *Cidaris tribuloides* Bl., *Echinometra subangularis* Desml., and *Toxopneustes variegatus* A. Agassiz. *Cidaris tribuloides*, though very rare indeed along the coast of Brazil and represented in the collection of the Commission from there by only a few fragments, was exceedingly abundant at Fernando de Noronha, occurring there in thousands. It never seems to attain the size of the specimen from Cape Verde Isles, figured by Agassiz (op. cit., pl. 1*d*), and varies much in the length and strength of the spines, which are usually more or less overgrown with calcareous sea weeds. Though Fernando de Noronha is not 200 miles distant from the main coast, yet there intervenes a great depth of water, as ascertained by recent exploration; the marine fauna of the island, however, seems to be very similar to that of the coast.

The following list probably contains all the littoral species of *Echini* at present known from Brazil, all of which, with the

exception of *Strongylocentrotus Gaimardi* A. Agassiz, have been found by the Geological Commission. The localities at which they were obtained are so nearly those recorded by Mr. Alex. Agassiz in his Revision of this group, that it would be of no value to enumerate them again here. The species are as follows:

Cidaris tribuloides Bl.,
Arbacia pustulosa Gray,
Diadema setosum Gray,
Echinometra subangularis Desml.,
Strongylocentrotus Gaimardi A. Agassiz,
Toxopneustes variegatus A. Agassiz,
Hipponoë esculenta A. Agassiz,
Clypeaster subdepressus L. Agassiz,
Mellita testudinata Klein,
Mellita sexforis A. Agassiz,
Encope emarginata L. Agassiz.

It has been noticed that *Mellita testudinata* is brilliantly phosphorescent at times, when it has been removed from the water for a few hours, though it be still wet and apparently alive. The phosphorence is very apparent over the entire body. I am indebted to Mr. Alex. Agassiz for kindly examining and verifying the determinations of a collection of the above *Echini* sent to the United States, excepting *Mellita sexforis*, which has been found only very recently.

Art. X.—*Undulations observed in the tail of Coggia's Comet*, 1874; by L. Trouvelot.

On the evening of July 21, 1874, at $9^h\ 0^m$, the moon being in her first quarter, and the sky remarkably clear even close to the horizon, my attention was attracted by a bright ray of light darting from the northwestern horizon, way up in the constellations. Taking it for an auroral phenomenon, I went in for the spectroscope; but on my return, after a few seconds, to my disappointment I found no more trace of it. Soon, however, it reappeared, and darted up in an instant after the manner of certain auroral rays, and vanished again after ten or fifteen seconds. I then became aware of my error, and found with surprise that the phenomenon was taking place in the tail of Coggia's Comet, the head of which was then plunged under the horizon.

During the whole time that I observed this interesting phenomenon, I saw the comet's tail shortening and extending, lightning up and extinguishing like the rays of certain auroras. Extended undulations, rapid vibrations, ran along it in succession from the horizon to its extremity, giving it the appearance of a fine gauze wavering in a strong breeze. The pulsations and the waves of light were of unequal duration; some being rapid, while others lasted a longer time. For over one hour the comet's tail kindled and extinguished more than one hundred times; the extinction being sometimes so complete that

it was impossible to see any trace of the comet; while sometimes it became so bright, that in spite of the light of the moon, it could be distinguished easily in all its contours, even to its very extremity, which was then a little to the south of γ Ursæ Majoris.

Be it coincidence or not, at the moment that this phenomenon was occurring, a strong magnetic perturbation was going on in Cambridge, where the declination needle oscillated through an angle of 1° 27′, although no auroral light was seen; and by the kindness of Mr. Cleveland Abbe, of the Signal Corps, I learn that no aurora was reported for that night.

It is not a new thing to see vibrations and pulsations running along the tail of comets. Many observers have seen this phenomenon; among others Longomontanus, Vandelin, Snellius and Father Cysat, who are reported to have seen undulations taking place on the border of the Comet of 1618, as if it was agitated by the wind. Hevelius observed analogous motions in the Comets of 1652 and 1661. Pingré asserted that he distinctly saw in the long tail of the Comet of 1769, "des ondulations semblables à celles que les aurores boréales présentent."* According to Winnecke, from the 5th to the 12th of October, 1858, the rays forming the superior part of Donati's comet spread and contracted suddenly like the rays of the aurora.

Cambridge, Jan. 5, 1877.

Art. XI.—*Sudden extinction of the light of a Solar Protuberance;* by L. Trouvelot.

On the 26th of June, 1874, while making my daily observations of the sun with the spectroscope at the Harvard College Observatory, I saw an unusual phenomenon, which may be worth recording. The narrow slit of the instrument was directed on the preceding side, about 270°, just above a group of spots which was then very near the limb, when I saw a brilliant protuberance partly projected on the spectrum, on the side of the rays of less refrangibility. In shape, this hydrogen flame resembled an elongated comma, having its acute extremity directed toward the sun, where it terminated just a little above the chromosphere. The chromosphere under this protuberance formed several slender and acute aigrette-shaped flames, none of which, however, reached it. The large prominence, which was slightly inclined to the limb, had a height of 3′ 37″, and about 3^{c} in its greatest width.

* Arago, Astro. pop., vol. ii, p. 439, Paris, 1855.

When the slit was set wide open, so as to allow the whole protuberance to be seen between its jaws, the comma-shaped flame appeared perfect, and showed plainly its texture. But when it was observed with a narrower slit it became partly invisible on the C line; only a short and jagged portion being seen in it on the red side. When the slit was carried along the protuberance by means of its screw, the portion visible on the C line did not remain constant, but either extended or contracted of a small quantity; the maximum portion visible on the C line never being more than one-fourth the width of the slit, while sometimes it was not seen at all on this line.

The portion of the protuberance projected on the spectrum was considerably more brilliant than the spectrum itself, and about one-third only of its whole length was visible. As the slit was carried along it, the visible parts became invisible near the C line, and invisible parts appeared on the spectrum; and the area of the visible portion either contracted or extended, when seen in different parts.

I had been observing this phenomenon for eight or ten minutes, when, while looking at it with the slit wide open, the flame suddenly vanished, at $10^h\ 30^m$, no traces of it remaining. As no motion of any kind, no extension, no contraction, could be perceived before or at the moment this phenomenon took place, and as the light did not go out of it gradually, but as suddenly as a flash of lightning, it does not seem that a change of position was the cause of its disappearance, but rather because the light which rendered it visible abandoned it in an instant.

According to theory, this protuberance was moving rapidly away from the earth at the moment of the observation, as it was projected upon the less refrangible side of the spectrum; yet this would fail to explain its sudden disappearance, since for this it should have moved out of sight with an unconceivable velocity.

For over half an hour I watched attentively the same spot in expectation of seeing the flame reappear; employing for this a narrow and a wide slit in succession, but with entirely negative results. I saw no more traces of it, although the small aigrette-shaped flames of the chromosphere, which were still visible, indicated the exact place where it had vanished, and where very probably it still existed, but now as a dark protuberance.

On several occasions I have seen the light abandon a protuberance gradually, but never so suddenly and on such a grand scale; and sometimes I have seen also the light gradually illuminating protuberances which were invisible before, something after the manner of clouds in our atmosphere, lighting up and fading into darkness by the appearance or disappearance of the sun. Of course, the illumination of dark solar protuberances

cannot be conceived as being due to the reflection of light, as in the case of the clouds in our atmosphere: it is the protuberance itself which is rendered luminous by some change taking place in it. These observations would seem to indicate that on the sun there are sometimes dark and non-luminous protuberances, which may cause the spots of absorption often observed in the vicinity of spots.

The phenomenon of the gradual illumination of a protuberance was observed in 1869, at Des Moines, Iowa, during the total eclipse of the sun, by Professor William A. Rogers, who accompanied Dr. C. H. F. Peters, on the Litchfield Eclipse Expedition. Professor Rogers was observing a large protuberance on the sun with a 9-inch-aperture refractor, when he saw several protuberances form gradually in the vicinity of the large flame, and at a considerable height above the chromosphere.

The projection of the hydrogen flames on the spectrum is not a very rare phenomenon during the period of maximum of sun-spots, and it has been observed several times. However, it may be worth while to record a characteristic case of projection, accompanied with remarkable changes of form, and a visible motion of the protuberance.

On Sept. 10th, 1872, at $12^h\ 33^m$, I was observing a small narrow flame forming an arch on the chromosphere, the height of which was equal to $36''$. Nothing remarkable was to be seen in this protuberance, although it was in the vicinity of a group of spots which was then very near the eastern limb of the sun; but, two minutes later, one of the extremities of the arch reposing on the chromosphere was suddenly detached from the limb, springing up like a distended bow, ascending in an instant to a height of $70''$, then appearing straight and rigid, but twisted like a rope. For a few seconds, it continued to ascend, at the same time growing wider; and at $12^h\ 37^m$, it had attained its maximum altitude of $118''$. It was then slightly curved. At $12^h\ 43^m$, the force which had carried it up began to give way, and it then descended rapidly toward the sun, folding upon itself in two places, while at the same time it became narrower. At $12^h\ 45^m$, it had reached its former height; and soon after it sunk to a level with the chromosphere and was lost in it.

At the same instant that the arc of hydrogen was distended, it was seen projected on the spectrum opposite the sun, towards the violet. The figure of this protuberance appeared exactly the same, whether it was projected on the spectrum or seen between the wide-open jaws of the slit. However, when the slit was narrow, the flame became invisible on the C line, although it remained projected on the spectrum. When the protuberance, after having reached its greatest altitude, de-

scended rapidly toward the sun, it remained projected on the spectrum just as before, although the descending motion was apparently in a contrary direction to the ascending one; but this did not seem to affect the position of the flame on the spectrum.

Cambridge, January 12, 1877.

ART. XII.—*The Moon's Zodiacal Light;* by L. TROUVELOT.

DURING the evening of April 3, 1874, the "Zodiacal Light" was particularly brilliant; especially close to the horizon, where it appeared as a segment of a circle, having an irregular wavy outline, giving it a vague resemblance to the beams of a faint aurora. Although the sky was clear, it was found impossible to observe with the telescope on that night, on account of the great disturbance of the atmosphere. At $9^h\ 45^m$, the declination needle indicated a very strong magnetic perturbation in Cambridge, oscillating through an angle of 3° 22′. However, no aurora was visible at this time, although the phenomena usually attending them were manifested during the evening by the tremulous appearance of the telescopic images.

While going home, I remarked in the east a strange conical light rising obliquely from the top of the roof of a building, behind which the moon, then about 15° or 20° above the horizon, was concealed from view. By going away from the building, the conical light, which closely resembled the tail of a comet, became brighter and brighter as it approached the moon, upon the western limb of which it rested. The base was at least as wide as the diameter of the moon; but it extended beyond, on each side, by a fainter light, which gradually vanished in the sky. The extension of this luminous appendage I estimated to be equal to eight or ten times the moon's diameter. It was not readily visible when the moon was in sight, as the brilliant light of our satellite overpowered its dim brilliancy. The axis of this appendage was found to be coincident, or nearly so, with the ecliptic; and its line prolonged in the west passed a little to the north of Jupiter.

The phenomenon had been observed for about fifteen minutes, when it gradually faded away until it almost totally disappeared five minutes later, although the sky was clear. A quarter of an hour after, the sky was overcast with dense vapors, which continued for nearly an hour.

At $11^h\ 0^m$ the sky had cleared up, and the moon shone brightly. The luminous appendage was still visible, and even appeared more brilliant than before. In order to ascertain

whether this appendage was visible only on one side of the moon, or if it was seen on the other side, I went under the piazza of my house, and placed myself in such a position as to have the moon concealed by its upper part, the sky below being visible. As I expected, a similar appendage was observed on the eastern side of the moon, exactly opposite the western one; the axis of both wings, passing through the moon's center, being in the plane of the ecliptic.

Although at this moment no auroral light was seen in the north, yet, up in the zenith, there were evident signs of it, as luminous vapors assembled there and rapidly dissolved, arranging themselves into bands radiating from a center after the manner of the crown of bright auroras. At $11^h\ 20^m$, all traces of the luminous vapors in the zenith had vanished; and at the same time the appendages of the moon were almost totally invisible, although the sky remained clear.

The fact that the zodiacal light had been unusually brilliant during this evening, and that the two luminous appendages of the moon resembled it in shape and appearance, and were situated in the same plane, seems to indicate that the two phenomena are of the same order; while the magnetic perturbation and the auroral phenomena connected with the variation of brightness observed in the moon's appendages would seem to indicate some kind of connection between the zodiacal light and the aurora. The result of my observations of the zodiacal light and the aurora during the last seven years also seems to indicate some such connection, as when the zodiacal light was observed to be particularly bright, it has generally been followed by auroral phenomena. But only a long series of observations in this direction can solve the problem.

Cambridge, Nov. 2, 1877.

ART. XIII.—*Chemical Equivalents and Atomic Weights considered as bases of a system of Notation;* by C. MARIGNAC.

(Translated from Moniteur Scientifique of Quesneville for September, 1877, by Mr. P. Casamajor.)

THE Academy of Sciences of Paris has witnessed lately, at several of its sittings, an interesting discussion, in which several eminent chemists, among its members, have taken part.* This discussion related to two questions which have often been brought before it, and which will probably be brought before it again many times.

One subject of discussion was a principle, stated in 1811 by

* Messrs. Sainte Claire Deville, Würtz, Berthelot, Fizeau.

an Italian philosopher, Avogadro, on the equality of the molecules of all bodies in a gaseous state. This principle is often placed in opposition to the law of Gay Lussac, on the simple relations which exist between volumes of gases, capable of combining with one another, which law was established a few years before, and which, to tell the truth, is not in contradiction with the hypothesis of Avogadro. From this question arose another, on the relative merits of the chemical notations, expressed in equivalents or in atoms.

For the present, I will not discuss the first of these questions. The truth of the principle of Avogadro can only be admitted on the condition of supposing that the atoms of simple gases cannot exist in a free state, but are welded together in pairs, forming molecules occupying two volumes, like the molecules of compound bodies. Exceptions should be made for mercury and cadmium, whose molecules are formed of only one atom, and for phosphorus and arsenic, whose molecules must contain four atoms.

This hypothesis is not absurd in itself. It may account for certain chemical facts, such, for instance, as the greater energy of action that bodies possess in a nascent state, or before their atoms have combined two by two to form molecules; also for the ease with which certain reactions take place, as pointed out by M. Würtz. It also explains several physical facts, such as the equality of specific heat for the same volume of simple or compound gases, whose molecule is formed of two atoms, as carbonic oxide, hydrochloric acid. It found lately an important confirmation in the researches of Messrs. Kundt and Warburg* on the specific heat of vapor of mercury, which show that this heat agrees with the mechanical theory of heat for monatomic gases, and that this agreement does not exist for other simple gases. We must acknowledge, however, that these considerations do not constitute sufficient proofs.

On the other hand, there are some compound bodies whose vapor densities are in contradiction with the principle of Avogadro. We should be forced to admit that all these compounds suffer decomposition when they seem to be reduced to vapor, so that, instead of measuring their volume, we measure that of their elements, or of the products of their decomposition. Although this decomposition has been ascertained in some cases, it has not in all.

As may be seen, the principle of Avogadro gives rise to serious objections, and, without being convinced of its worthlessness, like my eminent friend, M. Deville, I acknowledge that it is as yet but an hypothesis, in contradiction with facts, which have not been satisfactorily explained. But, I repeat

* Berichte der deutschen Chemischen Gesellschaft, 1875, p. 945.

it, I have no wish to enter into this discussion at present, as it would require to be extensively developed, and it can only be definitely settled by long and difficult experiments. I have recalled this discussion because its solution must exert a certain influence on chemical notations, although the connection between the two questions is not necessarily very close.

As to the best system of notations, it may be necessary to explain why such a question is raised and can only be raised in France. In every other country the question has solved itself gradually, as chemists have, one after another, accepted the atomic notations, and given up the formulas by equivalents in their writings and in their teachings * In this gradual manner, in almost every country, by the successive assent of the great majority of chemists, atomic formulas have been substituted for the others without any formal struggle. In France, however, it has been very different. I am not aware whether the regulations of the University† forbid a professor from adopting the method of instruction which he thinks best, or whether teachers adopt a uniform system from the belief that otherwise their pupils would be placed in a relatively inferior position, if they did not adopt the system most in favor with examiners; at any rate, such an important change, as the introduction of a system of chemical notation, can only be generally introduced when it has been judged necessary, not only by the majority of teachers, but also by the Superior Councils, which govern the University, and, in these, chemists are not the only persons who have influence. It is easily understood that, under these conditions, the partisans of both systems wish to have them discussed in the presence of the scientific body which has the greatest authority, with the object of maintaining the established system, or of introducing the other.

To enter into this discussion, it is doubtless advisable, in the first instance, to define what is understood by these equivalents and these atomic weights, which are placed in opposition to one another.

As to equivalents, I see that M. Berthelot tells us that "their definition is a clear conception." Unfortunately he did not give this definition, and I confess that I do not know of any, at least of any that is precise and general. Doubtless when we compare, with one another, elements, such as chlorine, bromine and iodine, the definition of their relative equivalents is perfectly clear, as the term equivalent is its own definition. But when we deal with bodies which have not a similar

* Fresenius still considers the formulas in equivalents as the best.—*Translator.*

† The entire educational system of France is consolidated under one organization. called the University, comprising faculties of letters, medicine, law and theology, *lycées* for secondary instruction and schools of primary instruction.—*Translator.*

analogy, and particularly if they do not perform the same functions, the idea of equivalence has no meaning. I defy anybody to give a general definition of equivalents which justifies the weight 14, adopted for nitrogen. In volume, it corresponds to the equivalents of hydrogen and of chlorine, but it has not the same chemical value. It has the same chemical value as the equivalents of phosphorus and arsenic, but it does not occupy the same volume. Moreover, it corresponds neither in volume nor in chemical value to the equivalents of oxygen, of sulphur and of most metals. Why then should this number exist?

If, instead of starting from a general definition, which does not exist, we try to find the meaning of equivalents in the methods employed in determining them, we are led to the following conclusion:

It is proved by experience that we may assign to a body, be it simple or compound, various weights, multiples of the same number, and that these weights express the proportions according to which all bodies combine with one another. We may choose one of these weights to express the equivalent of the body. All combinations may then be represented as the union of a certain number of equivalents of the elements, and, if the equivalent is represented by a symbol (in general the first letter of the name of each element), combinations may be represented by formulas which are not generally complicated. This is, after all, the only condition required of equivalents, and hence the only general definition, although not very precise, which can be given is that the equivalent represents for every element or compound body one of the weights which may combine with other equivalents. Theoretically it matters little which of the weights is chosen. Practically, however, one of the weights is preferred, taking as a guide one of the following rules, which cannot be considered as very rigid, as they do not all lead, in all cases, to the same result:

1. When bodies are analogous, and have the same chemical character, their equivalents are represented by the weights which replace each other in analogous combinations. Let us note, however, that this rule is not followed for compound bodies, such as bases and acids, whose so-called equivalents are weights which often have very different values of combination, and we are thereby led to very singular anomalies of statement, such as these: two equivalents of alumina corresponds to three equivalents of magnesia; one equivalent of phosphoric acid to three of nitric acid, &c. In reality the fundamental principle of equivalents has been entirely abandoned for compound bodies, and, in its stead, a method has been adopted, which has been borrowed from the atomic

theory, by taking for their weights the sum of the equivalents of the elements which they contain.

I believe I am not in error when I affirm this, as M. Berthelot* says: "One equivalent of phosphoric acid corresponds to three equivalents of nitric acid, when it forms a tribasic phosphate.

2. Equivalents are chosen in such a way that compounds, which offer the greatest analogies, are represented by similar formulas. This principle served as a guide in determining the equivalents of aluminum and of copper. It is often in contradiction with the preceding. For instance, aluminum and magnesium, which are both powerful deoxidizing agents, do not replace each other in the proportions indicated by the equivalents adopted for these two metals.

3d. When neither of these rules is applicable, or when they lead to complicated formulas, the equivalent of a body is chosen in such a way as to give the simplest possible formulas for its most important combinations. This rule justifies the adoption of the equivalents of nitrogen, phosphorous, arsenic and of some other elements.

We may see by the above that the equivalents constitute a purely conventional and arbitrary system, without any scientific value.

The explanation I have given of equivalents is somewhat different from that which my illustrious teacher, M. Dumas, gave in his lessons of chemical philosophy. This eminent chemist took as his starting point the equivalents of bases, as determined by their true chemical equivalence, founded on the same quantity of oxygen contained in the base. The equivalents of acids are rigorously deducted from the weights necessary to neutralize an equivalent of base. Afterwards, he seeks to establish the equivalents of the elements by considerations which, he acknowledges, are often arbitrary. This method of determining equivalents however, has been either never adopted, or entirely abandoned, doubtless because it led to formulas which are inadmissible. I have given the meaning of equivalents, such as they have been adopted, and not such as they might have been.

We may now consider the definition of atomic weights. If the precise definition of equivalents is impossible, while their determination is comparatively easy, for they are adopted by arbitrary rules, it is the reverse with atomic weights.

* Meeting of the Académie des Sciences of June 4th, 1877. I cannot in any manner accept what he says, in the same place, that this equivalent of phosphoric acid corresponds to one equivalent of nitric acid in monobasic phosphates, or two equivalents in bibasic phosphates. To admit such expressions, we must deny to water the part that all chemists attribute to it in these salts, since the publication of Graham's researches.

If we refer to the fundamental hypothesis of the atomic theory, which supposes that the divisibility of bodies is not indefinite, but that they are formed by the agglomeration of excessively small but indivisible particles, or atoms, the theoretical definition of atomic weights is of the simplest, as they are the relative weights of these ultimate particles. But, however simple the definition may be, the determination of the weights is surrounded with great difficulties.

The hypothesis of the existence of atoms accounts in such a simple manner for that of chemically equivalent proportions for elements which play the same part, that we are naturally led, at first sight, to consider these proportions as representing their relative atomic weights, although this consequence is not rigorously necessary. It is evident, however, that as neither this consideration of chemical equivalence, nor any other consideration drawn from chemistry alone, has led to a complete and logical system of chemical equivalents, we cannot by such considerations be guided in the choice of all the atomic weights, and as these, on account of the hypothesis that is made on their nature, cannot be arbitrary, like equivalents, it has become necessary to study the physical properties of the elements and of compound bodies to find motives for this determination of the atomic weights. Among the properties which can be appealed to, the most important are the densities of gases and vapors, the specific heats and isomorphism.

I acknowledge that in some very rare cases these three orders of physical properties do not lead to the same result, and I agree with Mr. Berthelot that between these three data we must make a choice. I am, however, in complete disagreement from him in the conclusion that I draw from this. If he does not say so expressly, his whole argument proves that, in his opinion, no account is to be taken of these physical properties, when they disturb the usage established for weights that have been adopted for a long time in chemical notations. On the contrary, I think that great account should be taken of these physical properties, and that when they all agree we must have no fear of modifying a few formulas which have only long usage in their favor, particularly if the necessary modification is unimportant. If, moreover, the physical properties do not agree, it is necessary to study the facts with the greatest care, and see if, in some cases, a disagreement can be explained and then choose the weight which agrees the best with the general properties of the elements and its combinations.

Is it impossible to do this? The best proof that it is not, and that there is even no serious difficulty in determining the atomic weight which agrees the best with the physical properties, is to be found in this circumstance that there is no disagreement

among chemists, who accept this system of notation, as to the atomic weights, except for a few bodies that are not, as yet, sufficiently known; whose physical properties have not been sufficiently studied, and for which, besides, the idea of equivalents is quite as uncertain as that of atomic weights.

I am perfectly aware that the majority of chemists, who have adopted atomic formulas, believe that they are now able to give a rigorous definition of atomic weights. Starting from molecules, which they define as the smallest quantity of a body, simple or compound, which can exist in the state of liberty; admitting as an axiom the principle of Avogadro, which states the equality of volume of all molecules in a gaseous state, from which may be deducted their relative weights, they define the atom as the smallest quantity of a body which may enter into the composition of a molecule. This definition allows them to determine the atomic weights with certainty, at least for those bodies that enter into volatile combinations. I have not given great weight to this consideration because, not more than Messrs. Deville and Berthelot, do I regard the principle of Avogadro as absolutely demonstrated. But I wish it to be specially noticed that there is not, so far as I know, a single case in which the application of the above definition of atomic weight has been used to change an atomic weight previously obtained by considerations based on the physical properties. Perhaps I should except boron and silicon; but the atomic weights of these elements had never been considered as firmly established, nor indeed had their equivalents. This observation might be appealed to as the strongest proof of the accuracy of the definition of atomic weights, but I have no wish to admit it, as constituting a sufficiently sure base for the determination of atomic weights.

I have here to answer an objection, which I acknowledge to be serious, and which I believe is at the bottom of the opposition of M. Berthelot. The atomic weights rest on an hypothesis which has never been, and, in fact, can never be demonstrated, which many scientific men do not consider as verisimilar, that of the existence of atoms.

I am nearly ready to agree with M. Berthelot in his opposition, and I have certainly no idea of defending the atomic theory, but merely the chemical notations founded on the atomic weights. My answer to the objection stated above is that the existence of atoms is only useful in justifying the name of *atomic weights*, which, for my part, I would very willingly have replaced by any other. I know of no case in which an atomic weight has been determined by a method founded on the indivisibility of atoms; consequently we may consider atomic weights as entirely independent of this indivisibility.

In reality, I consider atomic weights, and I believe that many chemists agree in this, as being only equivalents, in the determination of which arbitrary conventions have been replaced by scientific considerations, based on the study of physical properties.

Let us now sum up the advantages that atomic notations present from this point of view.

For the elements, in the first place, the atomic weights represent equal volumes of all simple gases, so that their ratios of combination in volumes are directly expressed by atomic formulas, while the formulas in equivalents do not offer this advantage. This law presents some exceptions for vapors, in the cases of phosphorus, arsenic, mercury and cadmium, but the same divergence exists for equivalents.

Atomic weights are exactly proportional to the specific heats of simple gases, that are not liquifiable, which agreement does not exist for equivalents. According to the law of Dulong and Petit, the specific heats of the atoms of all simple bodies, either solid or liquid, are nearly the same, except for three bodies, carbon, boron and silicon, whose physical properties offer numerous irregularities, and in which the specific heat varies with the temperature in a manner unknown in other bodies. Equivalents do not offer this concordance. I will not insist on the objection raised by M. Berthelot, and founded on this, that the equality of specific heats of atoms is far from being absolute, as he was sufficiently answered by MM. Würtz and Fizeau. I will merely add that if we only admitted physical laws that are absolute, we should have to reject them all. Even the law of volumes of Gay Lussac would have to be dropped, as it has been ascertained that all gases have not the same coëfficient of expansion, so that the existence of simple ratios in combinations by volume are not strictly accurate.

As to compound bodies, the molecular formulas, based on the use of atomic weights, present the same advantages, perhaps to a higher degree, when we compare them to the formulas in equivalents.

The use of atomic weights allows us to simplify the formulas of a great number of compounds by dividing them by two. Particularly is this the case with organic compounds. Not only does the formula become simpler, but there is an important advantage gained, that the formulas of almost all compounds correspond to the same volume, which is double the volume of the simple atom. The only exceptions are for a very limited number of bodies, generally belonging to types of complex composition, such as salts of ammonia and of bases derived from ammonia; even for these it has not been proved that they are not regulated by any law, even if M. Deville is

right in thinking that the irregularities they present are not due to the decomposition of their vapors. On the other hand, the formulas by equivalents teach us nothing on the vapor densities of compound bodies, as their equivalents may correspond to two, four or eight volumes of vapor, perhaps even of six, if the old equivalent of silicon is kept, as is done by many of those who prefer the notations by equivalents. Molecular formulas also agree with the specific heats of compound bodies in the solid state. According to the law of Woestyn, molecular heats are proportional to the number of atoms contained in the molecule, which law has the same degree of approximation as that of Dulong and Petit. Formulas by equivalents do not show these properties.

Finally, the system of notation, based on atomic weights, gives the explanation of several cases of isomorphism which are incomprehensible with the notation based on equivalents. For instance, in the case of perchlorates and permanganates, and in the case of chloride and sulphide of silver when compared to protochloride and protosulphide of copper. I may also recall that it was by considerations of the same kind that I was led to discover oxygen in fluorine compounds of niobium, where its presence had not been suspected, and that the formulas of these compounds, expressed in equivalents, would never have suggested this idea.

In presence of these advantages, we may ask: what are those that are offered by the system of equivalents and its resulting notation? I believe I can indicate two.

In the first place, as the system is conventional, it does not of itself contain any necessary reason for changes, and it may remain invariable. As there was no serious motive for choosing the number fourteen as the equivalent of nitrogen, rather than seven, which would have given it the same volume as oxygen, or $\frac{14}{3}$ which would have accounted for its value of combination toward hydrogen and the metals, we may readily believe that there will never be a sufficient motive to replace it by one of these numbers. The determination of equivalents not being governed by any fixed rule, they will not be necessarily modified when we come to have a more accurate knowledge of the properties of bodies.

In the second place, as, in their determination, no account is taken of the physical properties of bodies, greater attention can be given to their chemical equivalence, when it exists. This presents some advantages in practical chemistry.

These considerations are doubtless of some value; but if we examine things a little closer, we may easily see that, in this respect, there is really very little difference between the two systems.

It is true that there was a time when atomic weights had to be changed, and it is doubtless, on this account, that atomic weights were dropped and equivalents adopted. Nevertheless, the history of chemistry shows that for more than thirty years no changes have been judged necessary for well known bodies, and that those which have been admitted for elements, whose properties or whose combinations had previously been imperfectly known, were so thoroughly justified by their chemical properties, that even the equivalents of these bodies have had to be modified. Such was the case for bismuth, uranium, vanadium, tantalum and niobium. In reality, the only important change that atomic weights have had to suffer, since their introduction in chemical science, has been the reduction to half of the weights of silver and of the alkaline metals, a reduction based on their specific heat in the solid state; on the specific heat of their combinations, or on isomorphism as was done in the first instance by M. Regnault.* We may see by this that, on the score of invariability, the two systems are on a par.

As to the advantage which results from the fact that equivalents express ratios of real chemical equivalence, in cases where they are not indicated by atomic weights, it would be an important one if chemical equivalence were indicated in all cases; but we know that this is not so. It is really not more difficult to conceive and to remember that an atom of oxygen is worth two of chlorine, and an atom of lead two of silver than to know that an equivalent of nitrogen is worth three of oxygen, and that two equivalents of aluminium are worth three of magnesium. So there is really no advantage, on these two heads, which can counterbalance those which I have shown for atomic notations.

It may be said that the preceding is a contradiction of what I said before. I said that the system of equivalents presents conditions of invariability that are not presented by atomic weights. Further on I have shown that every change of atomic weight had necessitated a corresponding change in equivalents.

If we look for the cause of this apparent contradiction, it seems to me that we shall be led to make an observation which gives the key to the discussion actually going on. It is that, in reality, if we keep out of sight every question as to the *origin* of the terms *equivalents* and *atomic weights*, there is no difference between the two systems, and the partisans of equivalents are willing enough to accept the principles which serve to determine atomic weights, except when the necessity arises of changing the formulas of bodies that are of great importance and occur with great frequency.

* Annales de Chimie et de Physique, 1841, III, vol. i, p. 191.

ART. XIV.—*Some thoughts on the Glycogenic function of the Liver and its relation to vital force and vital heat;* by JOSEPH LECONTE.

[Read before the National Academy of Sciences, New York, October, 1877.]

THE great size of the liver and its persistence as a conspicuous organ, as we go down the animal scale even to a very low position, clearly demonstrate the great importance of its functions. This conclusion is entirely confirmed by the very grave effects on the health produced by its disorders. But in spite of its acknowledged importance, great obscurity still hangs about the true nature of its functions. The function of the liver is not simple, like that of the lungs or the kidneys, but very complex. The liver is the manufactory of both bile and sugar. The bile is both a *secretion* used in the digestive preparation of food, and an *excretion*, separating poisonous matters from the blood. The sugar, too, has doubtless as many and as complex uses; but these are little understood. Evidently, therefore, any light, even the smallest, thrown upon this sugar-making function of the liver will be hailed with pleasure both by the scientific physiologist and by the medical practitioner.

What I have to offer on this subject, however, is not the result of any elaborate research, nor of the discovery of any new facts, but simply the result of thought on, and necessary deduction from, facts already known. Neither are all the thoughts and deductions entirely new, but I cannot find that they have been held sufficiently firmly and stated sufficiently clearly by physiological writers, and for this reason, perhaps, have not borne any fruit in medical practice.

It will not be amiss to review very briefly the main and usually acknowledged facts connected with this function.

1. The portal blood of *flesh-fed* animals contains no sugar, but the same blood, after passing through the liver, i. e., the blood of the hepatic vein, contains always a notable quantity of this substance. Evidently, therefore, *it is manufactured in the liver.*

2. If the dead liver removed from the body be washed out completely by water injected through the portal and hepatic veins until every trace of sugar is removed, and then the liver be allowed to stand a while, on recommencing the transmission of water the first that passes is decidedly sugary. The same process may be repeated several times with the same result, until the material out of which the sugar is made is finally exhausted.

3. If the liver of any animal be kept a considerable time before cooking, the amount of sugar which accumulates in its

substance is so large as to be easily detected by the taste. The liver is *decidedly sweet.* I have very often detected this sweetness. I have not seen attention drawn to the fact.

4. Evidently, therefore, the liver is constantly forming sugar from some *insoluble* or *feebly soluble* substance, which on account of its feeble solubility cannot be easily washed out. This substance has been isolated by Claude Bernard and others, and its properties determined. It is a white, feebly-soluble, tasteless, amorphous substance, having the composition of starch or dextrin. It changes easily and rapidly into sugar by mere contact of blood or other albuminoid ferment. It is called glycogen or sugar-maker. It is a kind of *animal starch.* The quantity of glycogen in the liver varies much with the food, being greatest (17 per cent) with amyloid food, and least (7 per cent or even only 2 per cent) with albuminoid food. (Pavy.)

5. The sugar formed from glycogen (liver-sugar) is closely allied, perhaps in composition identical, with glucose; but apparently differs from this and all other forms of sugar in being more unstable, i. e., more easily fermented and especially *more easily burned or oxidized in the blood.* It therefore probably differs from glucose in *molecular structure* if not in chemical composition. Sugar in any other form than that made in the liver, may circulate for some time in the blood without oxidation, but liver-sugar is speedily burned. The great importance of this difference is usually overlooked.

6. This change from glycogen into liver-sugar—this manufacture of sugar in the liver—is *not a vital but a purely chemical process.* It takes place in the *dead* just as well as, or even much better than, in the *living* liver.

Such are the undoubted *facts* in regard to the formation of sugar in the liver. But the physiological *uses* of this function have remained in a large measure at least, a mystery. There are, however, some other equally undoubted facts which are believed to throw light on this point, and which therefore we will briefly state.

1. The amount of amyloids which may be taken, and often is taken, as food, by a healthy man in the course of a day, is certainly one or two pounds. The whole of this is converted into sugar before it can be taken up. Two pounds of sugar, therefore, may be taken into the blood of a man who is fed largely on amyloid food.

2. This amount of sugar in the blood would make that liquid as sweet as syrup. But so far from this being the fact, sugar is an extremely minute, a scarcely detectable ingredient of the blood of the *general circulation;* so much so, indeed, that it is rarely put down among its normal constituents. What

then becomes of it? Burned up, it will be answered. Yes, it is doubtless burned up; but this combustion occupies at least twenty-four hours, and in the meantime a large though gradually decreasing quantity ought to be found in the blood unless it is disposed of in some other way. How is it disposed of?

3. The dissolved food is presented to the absorbents of the stomach and intestines in three principal forms, viz: as *peptones* (or dissolved albuminoids), *sugars* (dissolved amyloids) and *fats*. Of these the sugars are taken up mostly by the *capillaries* of the stomach and intestines (portal capillaries), the *peptones* partly by the capillaries and partly by the lacteals, and the fats wholly by the lacteals. Nearly the whole of the sugar and a large portion of the peptones, therefore, pass into the portal vein and thence by a second capillary circulation are distributed through the liver before they can enter the general circulation. The fats, on the contrary, with much of the peptones and some of the sugar are carried by the thoracic duct and discharged directly into the general circulation at the junction of the subclavian and jugular veins and thence are carried immediately to the heart to be distributed everywhere.

Now, put these three facts together: (1) the large amount of sugar taken up from the stomach and intestines of amyloid-fed animals, (2) nearly the whole of this sugar is distributed through the liver before it can reach the general circulation, (3) the minute proportion of sugar found in the general circulation, and the conclusion seems unavoidable, viz: *The whole of the sugar is arrested in the liver, changed into a less soluble substance nearly related to sugar, viz: glycogen, and thus withdrawn from circulation and stored in the liver. The stored amyloid is then slowly re-changed to sugar, but in the more oxidable form of liver-sugar,* and re-delivered, little by little, to the blood by the hepatic vein, as the necessities of combustion for animal heat and vital force require.

The views thus far presented have been held with more or less firmness by most physiologists since Bernard's discovery of this function. In what follows I have attempted only to push these views to what seems to me their legitimate conclusions.

It will be observed that there is here a double change in opposite directions. The liver seems to change sugar by dehydration into glycogen only to change by rehydration glycogen back again into sugar. At first sight there seems to be, therefore, a waste of force such as never occurs in the animal body. This has been urged by Pavy and others as an objection to this double change, as a fact. But there are at least two good reasons for this double change. First: *A large quantity of sugar* (glucose) *in the blood is very hurtful.* The experiments of Dr. Wier Mitchell, of Philadelphia, have proved that large

quantities of sugar in the blood produce, among other hurtful effects, *cataract* and *blindness*. The cataract, so common among diabetic patients is doubtless due to this cause. Plainly, therefore, there is needed a *reservoir* to receive and detain the flood of glucose poured into the blood after every full meal of amyloids. The liver is that reservoir. A second reason for this double change—(and this is probably the fundamental reason)—is that the sugar is re-delivered to the blood not as glucose but as liver-sugar, and therefore *in a more oxidable form—as a better fuel* than previously.

For the sake of greater clearness, we have spoken thus far as if glycogen were made only from sugar. Most of it is undoubtedly made in this way in all animals except carnivora. But it seems certain that the *liver has the power of making glycogen from albuminoids also;* for animals fed on albuminoids alone still continue to make sugar from glycogen. Therefore—and this is a very important point—*albuminoids are decomposed in the liver into glycogen and some nitrogenous matter*, which is separated and excreted partly in the bile, but probably mostly restored to the blood to be excreted as urea by the kidneys. In this way the excess of albuminoid food, *over and above what is necessary for tissue-building is reduced to a condition suitable for easy combustion.*

But if this view be correct—if this be the way in which albuminoid *food* in excess of the requirements of tissue-building is disposed of, then it is certain that *waste tissues also* are eliminated in the same way, for these are also albuminoid. The first step in the elimination of these takes place in the liver where they are decomposed into glycogen to be burned as sugar and eliminated through the lungs, and a nitrogenous residue to be eliminated mostly by the kidneys. If so, then animals starved to death ought to make glycogen and liver-sugar to the last. Now, according to Chauveau, horses and dogs after six days fasting still continued to make liver-sugar.*

Again: we have spoken thus far only of the dissolved food taken up by the portal capillaries and distributed through the liver before entering the general circulation. By far the larger part undoubtedly takes this course; but a small portion also is taken up by the lacteals. This enters the general circulation through the thoracic duct, little by little, in proportion as it is taken up. But this also must pass, though not so directly, through the liver by the hepatic artery or by the mesenteric artery and portal vein and is doubtless also arrested there in the form of glycogen.

There are, then, *three sources* of glycogen, and therefore of liver-sugar, viz: 1. *The whole of the amyloid food.* 2. *All*

* Comptes Rendus, vol. xliii, p. 1008.

albuminoid food in excess of what is required for tissue-building; and 3. *All waste tissues.* But since in the mature animal waste tissue is exactly balanced by tissue-building, the sum of the last two sources is exactly equal to the whole albuminoid food; and the material used for glycogen-making is, therefore, equivalent to the whole food, unless we except fats. The amyloid material is changed *without loss*, but the albuminoid material is changed with *large loss of nitrogenous residue*, which is eliminated partly as the nitrogenous matter of the bile, but mostly as urea by the kidneys. There are also the *same three sources of vital force and vital heat*, viz: 1. Combustion of amyloid food. 2. Combustion of the combustible portion of albuminoid food-excess. And 3. Combustion of the combustible portion of waste tissues. And, for the reason already assigned, this is exactly equivalent to combustion of the combustible portion of the whole food. Therefore, *the sole object of the glycogenic function of the liver is to prepare food and waste tissue for final elimination by lungs and kidneys; to prepare an easily combustible fuel, liver-sugar, for the generation of vital force and vital heat by combustion, and at the same time a residuum suitable for elimination as urea.* And conversely: *the only combustible used in the body is liver-sugar*, unless, indeed, we except fats, the mode of combustion of which is not known. We at once see, then, the importance of this function, lying, as it does at the very foundation of all the eliminative processes, as also of all force-making and heat-making processes of the animal body; and we are led to anticipate that the failure of this function must entirely sap the vitality of the system. That such is a fact we shall presently see.

There are several important conclusions to be drawn from the above to which I would next call attention.

1. The real function of the liver in this connection is not glycogenic or *sugar-making* as usually supposed, but glycogenic or *glycogen-making.* Glycogen-making is a true *vital function;* sugar-making is a pure *chemical process.* The former is an *ascensive*, the latter a *descensive*, metamorphosis. The former can take place only in the *living* liver: the latter also in the *dead* liver, and even more rapidly, because the opposite force is withdrawn and the contrary process of fixing is stopped. In this as in so many vital processes we may, in the present state of our knowledge, best represent the condition of things as the result of two opposing forces, a higher vital and a lower chemical; the former producing ascensive, the latter descensive changes. In death the latter triumph, and the changes are descensive only. These two opposite processes in the liver are fully recognized by Claude Bernard, although for convenience both processes are called glycogenic.

2. In the well-known and usually fatal disease, diabetes, sugar is excreted in large quantities by the kidneys. But the kidneys are not the organ in fault. On the contrary they do all they can to remedy the evil. Sugar in the blood is extremely hurtful; the kidneys remove it as fast as they can. Some have supposed that the lungs are in fault. The sugar contributed by the liver to the blood, they say, is not burned up as fast as received (as it ought to be) on account of deficiency of oxygen taken in by the lungs. But the true organ *directly* in fault is undoubtedly the liver. In diabetic patients the liver seems to have *lost its glycogen-making power.* The sugar taken up by the portal capillaries is not arrested in the liver, but allowed to pass straight through unchanged into the general circulation, whence it must, of course, be eliminated by the kidneys. The difficulty is not *too much sugar-making but failure to arrest the sugar as glycogen.* This is proved by cases of *traumatic* diabetes. It is well known that puncture of the floor of the fourth ventricle of the brain produces diabetes. In such cases, even though sugar be ingested in large quantities there is no glycogen formed in the liver; the glucose having been allowed (probably on account of paralysis of the vaso-motor nerves) to pass straight through unchanged into the general circulation. The extreme gravity of *pathological* diabetes, therefore, is partly the result of the directly hurtful effects of sugar in the blood, as shown by diabetic cataract, but chiefly owing to serious disorder of a very important function of the liver—a function on which wholly depends animal heat and animal force of all kinds, viz: the preparation of food and waste-tissue for easy combustion. The use of a pure albuminoid diet may remedy the former difficulty, i. e., the presence of excess of glucose in the blood, but cannot remedy the latter and greater difficulty. We at once see, therefore, why an extremely low vitality and a temperature which is subnormal, even in fever, should characterize this disease.

3. There is a very interesting analogy between this glycogen-making function of the liver and the *starch-making function of plants.* Plants change the soluble forms of amyloids, viz: dextrin and sugar into the insoluble form of starch. They do so for the purpose of storing it away for *future use.* When ready to use it they again reconvert it into the soluble form of dextrin or sugar and take it again into circulation. It cannot be *stored* unless in the *insoluble* form; it cannot be *used* unless in the *soluble* form. So animals also change soluble sugar into insoluble glycogen (animal starch) and store it away in the liver for future use. In order to use it, however, it must be reconverted into the soluble form of sugar.

In a work entitled "Digestion Végétale," published in 1876,

M. Morren likens the change of starch into sugar in plants to digestion in animals, and concludes that the function of digestion can no longer be regarded as peculiar to animals, but is universal among organisms. But, as we have already shown, the true analogue both of starch-formation and starch-solution in plants is found in the glycogenic function of the liver. Digestion is properly a solution of *solid food*, preparatory to its absorption into the circulation. No such function is needed in plants (unless we except insectivorous plants) because their food is already liquid. Materials already absorbed and stored in the tissues cannot be called food, nor can changes in such materials be called digestion except by a metaphor.

4. The analogy between animals and plants as regards this function of storing amyloids is far more striking in the lower animals and in embryonic conditions. All parts of plants seem to have the power of fixing and storing amyloids, although it is mostly confined to parenchyma or undifferentiated cellular tissue; while in the higher animals this function is confined to the liver. But it is an interesting fact showing the greater resemblance of lower animals and of the embryonic conditions of higher animals, to plants, and illustrating the law of differentiation both in the development of the embryo and in the evolution of the organic kingdom, that the embryos of the higher animals and the adults of many lower animals, have also the power of fixing amyloids or making glycogen, *in all parts*, although this is especially true of undifferentiated *epithelial tissue* (Bernard.) And, what is very noteworthy, it is *sluggish* animals, or animals least endowed with nervous energy, which accumulate most glycogen in their tissues. Bizio found 10–14 per cent in the *dried* tissues of the oyster and cardium. Foster fully as much or more in entozoa, viz: two per cent wet weight. Thus as we pass up the animal scale not only do we find this function more and more localized in one organ, the liver; but also the glycogen product more and more quickly consumed by combustion for animal activity.

5. But if there are striking resemblances, so also are there differences in the use of stored amyloids very characteristic of the two kingdoms. Plants store away starch for future use as *building material:* of course, therefore, after re conversion into sugar it must be again re-converted into insoluble cellulose. Animals on the contrary store away glycogen for future use as *fuel for force-making.* Plants do not require it for force for *they draw their force from the sun.* Animals do not require it for tissue-building *for their tissues are wholly albuminoid.*

6. We have seen that albuminoids, whether food or waste tissues, are probably split in the liver into glycogen and some nitrogenous residuum. The glycogen is changed into sugar and

then by oxidation into CO_2 and H_2O and eliminated by the lungs. The nitrogenous residuum if it is not at first urea is at least easily changed into urea and eliminated by the kidneys. We see then the *close relation* between the functions of the liver and kidneys. Now in going down the animal scale, we find in many cases, as for example in insects, *that the same organ performs both functions.* In the process of evolution urea was at first formed and excreted at once by the same organ. Afterwards these two processes were differentiated.

7. We have already stated that Foster found a large percentage of glycogen in the tissues of entozoa.* He asks, "Of what use is this glycogen?" and answers, "No respiratory use" is possible, for "having a constant temperature secured to them" by the body of their host, "they are the very last creatures to need respiratory or calorifacient material." He, therefore, inclines to the view of Pavy, that glycogen is not respiratory material at all, but material on its way to become fibrin. On the contrary, *we*, for our part, see in this observation of the presence of stored amyloid in an animal not needing any internal source of heat, a striking proof (if any were still needed) that the prime object of respiration is not *heat-making but force-making;* and the view of Foster we regard as a no less striking proof of the fact that this fundamental principle of physiology is even yet imperfectly recognized by some of the best men. The very use of the term heat-making material as synonymous with respiratory material shows a profound misconception. But this expression as applied to amyloids and fats has unfortunately become so universal, and is now so hedged about by authority of great names that it is almost impossible to eradicate the false ideas associated with it. It cannot be too often or too strongly enforced that the prime object of combustion in the animal machine, as in the steam-engine, is not heat-making but *force-making.* Heat is only a *concomitant*, often useful, but sometimes useless and even distressing. Careful experiments have over and over proved this; the doctrine of conservation of force absolutely requires it; and yet physiologists still go on serenely talking of respiratory food as only heat-making, as if vital force were something wholly unrelated to other forces of nature and therefore requiring no explanation at all. The vital force created by combustion may be expended largely in mechanical power, as in the higher animals, or almost wholly in the vegetative functions of digestion, assimilation, secretion, &c., as in the more sluggish lower animals such as many mollusca and entozoa.

Postscript.—Since finishing this article in April last, I have read with deep interest a very remarkable paper by Professor

* Proc. Roy. Soc., xiv, 543.

Schiff,* entitled "A new function of the liver," in which the author demonstrates in the most convincing manner by means of experiments on dogs and frogs conducted in the physiological laboratory in Geneva, that *the liver has the power of completely decomposing poisonous matters generated by the wasting of tissues.* Ligation of the vessels of the liver, especially of the portal vein, in mammals, quickly produced extreme lethargy and finally death in the course of one to three hours. In frogs, it is true, ligation produced little effect, but this is only because of the slowness of the changes in their tissues; for the blood of dogs dead of ligated livers injected into the circulation of frogs quickly destroyed life if their livers are ligated, but produced no effect if the livers are unligated. Moreover many other organic poisons were shown to be either wholly or partly destroyed and rendered innocuous by the liver. Doubtless this is the true explanation of the more potent effect of organic poisons when administered by subcutaneous injection; for they then enter the general circulation without passing through the liver.

I regard these experiments as a most striking confirmation of the view presented in this paper, viz: that waste tissues are decomposed in the liver into glycogen and a nitrogenous residuum eliminated mostly by the kidneys. But if my view as to the mode of decomposition be correct, then the experiments of Professor Schiff have demonstrated not a *new function*, but rather an extension of the glycogenic function, throwing upon it a *new light* and giving it a *new significance.* It is precisely this new significance which I have attempted to set forth in the foregoing paper.

Berkeley, Cal., Oct. 15, 1877.

ART. XV.—*Revision of the Atomic Weight of Antimony;* by JOSIAH P. COOKE, Jr.

(Concluded from page 49.)

AFTER a long series of experiments, which threw but little light on the cause of the discrepancy that had now become so prominent, although they yielded very interesting subsidiary results which are described in detail in the original paper, we made a series of analyses of antimonious bromide with the hope that if there was, as we suspected, a source of constant error in the analyses of anitimonious chloride, the same influence would affect the analyses of the corresponding bromide to a different degree and might thus reveal its nature.

* Arch. des Sciences, lviii, 293, March, 1877.

We prepared the bromide of antimony by adding in small portions at a time the pulverized metal to a strong solution of bromine in sulphide of carbon. The retort containing the solution was kept cool by snow, and shaken after each addition until the action ceased. As soon as the color of bromine was discharged, the sulphide of carbon was distilled off over a water bath; and then, replacing the water bath with a gas lamp, the bromide of antimony was first boiled, and then distilled over the finely powdered antimony which had been added in excess. On account of the high boiling point of bromide of antimony, and the readiness with which its vapor condenses, it was found best in distilling to cover the body of the retort with a hood. The bromide thus prepared was purified by repeated distillations over pulverized antimony, as in the case of the chloride, and finally by crystallizing and recrystallizing several times from solution in purified sulphide of carbon. A warm saturated solution in sulphide of carbon deposits, when cooled to the freezing point, the greater part of the bromide of antimony in fine acicular crystals. These crystals were dried first with blotting-paper, and then *in vacuo* over sulphuric acid. The antimonious bromide thus purified by fractional distillation and crystallization was only a very small fraction of the first crude product. It was pure white, had a high silky luster, and, when first made, was wholly destitute of odor. It was carefully examined for chlorine, iodine, and arsenic; but the delicate tests which we possess for all three of these elements so frequently associated with commercial antimony and bromine, failed to show the least trace of either in the bromide of antimony we analyzed. The determinations of bromine were made in all respects like those of chlorine. Great care was taken not to add more than a very slight excess of argentic nitrate, and we found that under these conditions the supernatant liquid cleared more readily above the precipitate in the case of bromide of silver than with the corresponding chloride, and for this reason the first could be washed more quickly than the last. The results of these determinations are embodied in the table on the following page.

Here, as before, the letters indicate different preparations: *a* was made and purified as described above; *b* was the same material as *a* redistilled and again crystallized from bisulphide of carbon; *c* was another portion of the same material several times redistilled and twice recrystallized from the same solvent; *d* was a separate preparation from the start; *e* was another separate preparation purified with extreme care. In the last case there was over a kilogram of the crude product, which was reduced by the fractional distillation and crystallization—each process repeated from ten to twenty times—

to the few grams used in the analyses. These methods of purifying the substance were thus pushed to their utmost limits.

ANALYSES OF ANTIMONIOUS BROMIDE.

DETEMINATION OF BROMINE.

No.	Wt. of $SbBr_3$ taken in grams.	Wt. of AgBr obtained.	% of Bromine Br=80, Ag 108.
1, *a.*	1·8621	2·9216	66·765
2, *a.*	0·9856	1·5422	66·584
3, *b.*	1·8650	2·9268	66·779
4, *b.*	1·5330	2·4030	66·703
5, *b.*	1·3689	2·1445	66·663
6, *c.*	1·2124	1·8991	66·655
7, *c.*	0·9417	1·4749	66·647
8, *d.*	2·5404	3·9755	66·593
9, *d.*	1·5269	2·3905	66·623
10, *e.*	1·8604	2·9180	66·743
11, *e.*	1·7298	2·7083	66·624
12, *e.*	3·2838	5·1398	66·604
13, *e.*	2·3589	3·6959	66·671
14, *e.*	1·3323	2·0863	66·635
15, *e.*	2·6974	4·2285	66·708

Mean value from last six determinations,	66·664
Mean value from all the determinations,	66·6665
Theory Sb 120 requires,	66·6666
Theory Sb 122 "	66·2983

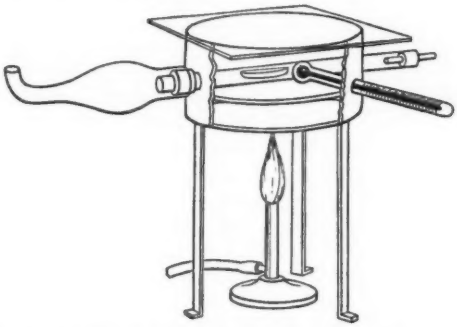
If in calculating the result of the above bromine determinations we use the atomic weights of Stas,—Br=79·952, Ag=107·93,— the per cents found will be in each case only 0·002 higher, which is, of course, an inappreciable difference. Hence, whether we take Stas's or Dumas's values for the atomic weights of bromine and silver, the atomic weight of antimony deduced from the above determinations is exactly 120·00.

This is certainly a remarkably close confirmation of our previous conclusion. Indeed the wonderful coincidence between the observed and the theoretical results must be to a certain extent accidental; for no process of chemical analysis is capable of the accuracy which this agreement would imply. Still it should be noticed that the probable errors of the process, so far as they are indicated by the variations from the mean value, are not larger than we might expect would be eliminated by multiplying observations; and, further, that the mean of the last six determinations which are undoubtedly the most trustworthy, is nearly as close to the theory as the mean of the whole.

But not only did these experiments on bromide of antimony thus confirm our previous conclusion: they also gave the first definite clew to the explanation of the disagreement with otherwise consistent results which our experiments on chloride of antimony had presented. The one difference between the chloride and the bromide, which appeared to render the last better suited to yield accurate results, was the difference in their hygroscopic qualities. As we have stated, the chloride is one of the most hygroscopic substances known. The bromide is also hygroscopic, but far less so, presenting no unusual difficulties of manipulation; and, since our tests indicated that both substances were otherwise pure, we at once drew the inference that the different results we had obtained with chloride of antimony must depend on the extraordinary attraction of this substance for moisture. Before, however, fully following out the clew thus obtained, we made a similar study of the iodide of antimony.

The iodide of antimony was prepared like the bromide, by shaking up in a glass flask a solution of iodine in bisulphide of carbon with finely pulverized metallic antimony. On filtering and decanting, after the color of the iodine is discharged, a solution having a pale greenish-yellow color is obtained, from which on cooling or on evaporation red crystals of iodide of antimony are deposited. The substance may be purified by recrystallization from the same solvent; but iodide of antimony is far less soluble in bisulphide of carbon than the chloride or bromide, and cannot therefore be so advantageously treated in this way, nor can the small amount of carbonaceous impurity which the crystals acquire from the solvent be so easily removed. Moreover, iodide of antimony cannot be so readily distilled as the chloride or bromide, on account of its high boiling point, which is above that of metallic mercury. But another property of iodide of antimony which, so far as we know, has not hitherto been noticed, interferes still more seriously with these methods of purifying this substance. In all its conditions, it undergoes a more or less rapid oxidation in contact with atmospheric air, forming oxi-iodide of antimony (SbOI) and free iodine. When iodide of antimony is rapidly boiled in a small flask, so that the body and most of the neck are kept full of vapor at the boiling-point, the action at the surface of contact of the vapor and the air is very striking; iodine is set free in vapor, with its familiar violet color, while the oxi-iodide is precipitated in clouds, forming a most beautiful phenomenon. So also when the greenish-yellow solution (above described) of the iodide in bisulphide of carbon is exposed to the air and light, it rapidly becomes colored red from the liberation of iodine, and at the same time turbid from the

deposition of the insoluble oxi-iodide. Even the crystals of iodide of antimony, when kept in the light, slowly become opaque from the formation of the same oxi-iodide; while the odor and staining of the stopper of the bottle furnish abundant proof of the liberation of iodine. The study of these phenomena was most interesting, and the results obtained will be described in another paper. It is sufficient for the present to say that they pointed out to us a great source of impurity in iodide of antimony, and fully explained the want of accordance in our analyses of the crystals of this substance as first pre-



pared. It was evident that we could only hope to purify the material by distilling or subliming it in an atmosphere of inert gas; and we devised the apparatus represented in the accompanying figure for this purpose, which we have since found very generally useful for all sublimations where the temperature required does not exceed that which can be measured with a mercury thermometer. The apparatus has been already referred to, and requires no further description. It was a simple modification of the apparatus used before for drying at a regulated temperature the precipitates of sulphide of antimony, which, as we have stated, was so arranged that the character of any sublimates which might be given off could be observed. We used the same glass tube passing through the sheet-iron air-bath, with its transparent mica cover, only we added a common glass adapter, selected so that its mouth just fitted over the open end of the tube. A platinum nacelle containing iodide of antimony, which had already been purified by crystallization, was placed in the tube within the air-bath, but near the open mouth; and, while a current of dry carbonic dioxide through the apparatus was steadily maintained, the air-bath was heated by a gas lamp to the required temperature which was indicated by a thermometer as shown in our figure. Iodide of antimony is sensibly volatile, even at 100°; and long before

it reaches its melting point, 167°, the evaporation becomes very marked. As soon as melted, it sublimes quite rapidly; and we obtained the best results by keeping the temperature between 180° and 200°, and by shifting the adapters we used as receivers, it was easy to collect the different portions of the sublimate. We thus obtained crystals of two isomeric modifications of iodide of antimony: the more abundant in large hexagonal plates, often half an inch or more in diameter, perfectly transparent, and of the most brilliant ruby-red color; the other in small rhombic plates, having the same peculiar greenish-yellow color as the solution of the iodide already mentioned. The amount of the last was always small, but it was larger in proportion as the temperature was lower. This new and most interesting product will be described in the paper just referred to. Of these crystals, the most brilliant, chiefly of the red variety, were selected for analysis. The iodine determinations were conducted in all respects like those of chlorine and bromine. The iodide was first dissolved by a very concentrated solution of tartaric acid, and then the solution was diluted to the required extent. The same care was taken not to add more than a very slight excess of argentic nitrate, and the amount required was accurately weighed out in each case. Each of the determinations was made with a separate preparation in so far as it was a product of a separate sublimation; but the material sublimed was essentially the same in all cases,—a mixture of the products of many crystallizations from the crude material made as described above. The results are collected in the following table:

ANALYSIS OF IODIDE OF ANTIMONY.

IODINE DETERMINATIONS.

No.	Wt. of SbI_3, grams.	Wt. of AgI, grams.	% of Iodine, I=127, Ag=108.	Variety.
1.	1·1877	1·6727	76·110	Pure red.
2.	0·4610	0·6497	76·161	Chiefly yellow.
3.	3·2527	4·5716	75·956	Pure red.
4.	1·8068	2·5389	75·939	Pure red.
5.	1·5970	2·2456	75·990	Red and yellow.
6.	2·3201	3·2645	76·040	Pure red.
7.	0·3496	0·4927	76·161	Chiefly yellow.

Mean value.....76·051
Theory Sb=120, requires..... 76·047
Theory Sb=122, " .....75·744

If in calculating the results of these iodine determinations we use the atomic weights of Stas, I=126·85 and Ag=107·93, the mean value would be 76·034, and the corresponding atomic weight of antimony 119·95.

The difference (0·004) between the first mean value and theory—corresponding to only about $\frac{2}{1\,8}$ of a milligram in the largest amount of argentic iodide weighed—is evidently insignificant, so that these results confirm the lower value of the atomic weight of antimony as closely as did the analyses of the bromide.

As we have already intimated, our analyses of the iodide of antimony, as first crystallized from bisulphide of carbon, yielded very discordant results. These we give in the table below, not, as before, in the exact order in which the analyses were made, but in the order of the several values, so as to exhibit the distribution of the errors.

ANALYSES OF CRYSTALLIZED ANTIMONIOUS IODIDE, RED VARIETY.

No.	% of Iodine.
1	75·71
2	75·76
3	75·78
4	75·80
5	75·84
6	75·85
7	75·87
8	75·89
9	75·94
Mean value	75·83
Theory Sb=122	75·74
Theory Sb=120	76·05

The cause of this discordance we attributed, as we have intimated, chiefly to the remarkable readiness with which iodide of antimony undergoes oxidation in contact with the air, resulting in the formation of oxi-iodide of antimony and free iodine, thus:—

$$2SbI_3 + O\text{-}O = 2SbOI + 2\,I\text{-}I.$$

While the free iodine escapes, the oxi-iodide remains as an impurity in the preparation, and the effect is a replacement of a portion of its iodine by oxygen. Now, since eight parts of oxygen replace one hundred and twenty-seven parts of iodine, it can readily be seen that an otherwise almost imperceptible amount of oxidation would be sufficient to produce all the variation from the normal composition which the above results present. A simple calculation will show that an absorption of only $\frac{4}{1\,8}\frac{7}{6\,8}$ths of one per cent of oxygen, or less than half a milligram by each gram of iodide of antimony, would reduce the per cent of iodine from the theoretical value, 76·047, to the mean of the above results,

75·832; and that a corresponding absorption of three-quarters of a milligram would reduce the per cent to 75·700, the lowest observed. It is not, therefore, surprising that we could obtain concordant results only with material which had been both purified by crystallization and also *recently* sublimed.

Returning now to discuss again the cause of the disagreement of the analyses of antimonious chloride with our otherwise consistent results in regard to the atomic weight of antimony, it was obvious that the strong hygroscopic power of the chloride must lead to a replacement precisely similar to that which is produced in the iodide by direct oxidation; for, as we have before said, the crystals of antimonious chloride cannot be exposed to the atmosphere for an instant without absorbing a perceptible amount of moisture, and every molecule of water thus absorbed reacts on a molecule of the chloride, thus:—

$$SbCl_3 + H_2O = SbOCl + 2HCl.$$

And when the antimonious chloride is boiled, the hydrochloric acid formed is given off, while the oxichloride remains behind, dissolved in the great mass of the liquid. Indeed, it seems impossible, with our ordinary appliances, to prepare or purify antimonious chloride without its becoming contaminated with oxichloride; and our experiments would indicate that when once it has been formed, as above described, in the mass of the material, it cannot be wholly removed by distillation or crystallization, however often these processes may be repeated.

Naturally, our attention was very early called to this obvious source of impurity in the antimonious chloride we prepared: and we noticed from the first that, even after the material had been many times distilled, there was always left, on repeating the process, a very small amount of dark-colored residue. We had examined the residue, and found that it was a mixture of chloride and oxichloride of antimony, colored by a trace of carbonaceous material; and we had made a long series of analyses for the purpose of studying the effect produced by the action we have described. The result of these analyses is given in the following table. We started with material already purified by fractional distillation and crystallization, and distilled it ten times in succession; not, however, carrying the distillation to absolute dryness, but leaving, so far as we could judge by the eye, about the same amount of residue in the retort each time. These residues we analyzed, as we did also the final distillate. The material first distilled was the same as that marked *c* in the table on page 47, and we assumed that the average of the results there given truly represented its composition.

ANALYSES OF ANTIMONIOUS CHLORIDE.

RESIDUES AND DISTILLATES.

	% of Chlorine.
The original purified preparation	46·64
The residue of 1st distillate	45·71
" 2d "	45·66
" 3d "	46·03
" 4th "	46·26
" 5th "	46·26
" 6th "	46·00
" 7th "	46·03
" 8th "	45·94
" 9th "	45·65
" 10th "	45·99
The last distillate	46·62

Although, under the circumstances, we could not expect great precision, yet it was evident from these analyses that the amount of impurity in the residues was not diminished by the successive distillations; and we therefore concluded that additional oxichloride of antimony must be formed each time during the very short contact with the atmosphere which the transfers between the several distillations necessarily involved. But, on the other hand, the very remarkable fact that these ten distillations produced no sensible change in the composition of the great mass of the material seemed to indicate equally clearly that this action of the atmosphere had no perceptible influence on the final result; and this opinion was still further strengthened when, on twice distilling portions of the last distillate, at a low temperature, in a current of dry hydrogen, we obtained products given again—very nearly at least—the same per cent of chlorine. And, lastly, when to all this evidence were added the results of the complete analysis of the chloride, showing an amount of antimony which fully supplemented the very constant per cent of chlorine, the assumption that any material amount of impurity could be present appeared wholly untenable. Yet we have seen how this assumption was forced back upon us by the subsequent results of the investigation.

Returning to the subject after our experiments with iodide of antimony, we, for the first time. fully appreciated how very small an amount of oxygen—the only real impurity present—was required to reduce the per cent of chlorine in antimonious chloride from 47·02, the amount corresponding to Sb=120, to 46·61, which corresponds to Sb=122; for, while the effect is so differently produced, yet the result of the action of the atmosphere on the chloride of antimony is wholly like that of its action on the iodide. It ends in replacing a small amount of chlorine by oxygen; and although, in consequence of the

smaller atomic weight of chlorine, it requires in this last case a larger replacement to produce a corresponding change of percentage composition, yet still the amount required to make all the difference in question is very small; so that, when we come to sum up the supposed completed results (as on page 47), it might easily be covered up by slight inaccuracies of the analytical work. An easy calculation will show that the substitution of but $\frac{146}{1000}$ of one per cent of oxygen for the equivalent amount of chlorine would reduce the per cent of this last element in the chloride from 47·020, corresponding to Sb=120, to 46·608, which corresponds to Sb=122; and such a substitution would result from the absorption of only $1\frac{6}{10}$ milligram of water by each gram of the chloride. The composition of the material would then be as follows:—

COMPOSITION OF ANTIMONIOUS CHLORIDE WITH $\frac{146}{1000}$ PER CENT OF O WHEN Cl=35·5 AND Sb=120.

Chlorine	46·608
Oxygen	·146
Antimony	53·246
	100·000

Now it will be seen by referring to the tables, on pages 47 and 48, that these percentages do not differ from the mean of the results of our previous analyses as much as these results differ among themselves; and we therefore determined to repeat these analyses, hoping that the experience we had acquired in both chlorine and antimony determinations would now enable us to obtain results sufficiently sharp to show even the small differences of composition which the substitution in question would produce.

Meanwhile, we instituted a series of experiments with a view of studying the decomposition which the oxichloride of antimony undergoes under the action of heat, in the hope that we might thus discover some method by which the amount of oxichloride of antimony in our preparations might be directly determined. For this purpose, we used first crystallized SbOCl, obtained by the action of alcohol on chloride of antimony in a sealed tube, which we weighed out into a platinum nacelle, and heated to various regulated temperatures, using for this purpose the apparatus already described. It appeared that the decomposition took place in two stages. The first stage of the decomposition began between 167° and 175°, but was not completed until between 260° and 280°. The second stage began at about 320°, but required for its completion a red heat. During both stages, chloride of antimony sublimed; and there was left in the nacelle at the close of the process beautiful crystals

of Sb_2O_3. In another experiment, we used crystallized $Sb_4O_5Cl_2$, prepared in the same way as the SbOCl, but with different proportions of alcohol and chloride of antimony. In this case, the decomposition did not begin until 320°, but in other respects both the process and the products were as in the first experiment. It was quite evident that the chemical changes which took place in the two stages of decomposition we have noticed were represented by the following reactions:

$$\text{First stage:} \quad 5SbOCl = Sb_4O_5Cl_2 + SbCl_3; \quad (1)$$

$$\text{Second stage:} \quad 3Sb_4O_5Cl_2 = 5Sb_2O_3 + 2SbCl_3; \quad (2)$$

but the relative weights observed in the first two experiments were of no value, because it was evident that a no inconsiderable amount of Sb_2O_3 was lost by sublimation. Since, however, the small sublimate of oxide condensed in the glass combustion-tube very much nearer the nacelle than the very much larger sublimate of chloride, we varied the apparatus in our third experiment so far as to place the nacelle in a tube of the shape represented in the accompanying figure. This tube was weighed with the nacelle, and was so selected that it quite closely fitted the combustion-tube within which it was placed for heating, as shown in figure by dotted lines. And it is evident that, while with this arrangement the $SbCl_3$ would be swept by the CO_2 gas into the colder portion of the combustion-tube, the greater part at least of the sublimed oxide would be retained in the small tube, which was of course at each stage weighed with the nacelle, as at first. Our results were as follows:—

Weight of SbOCl	0·4939 grams.
Loss at 280°	0·1271 "
Required by theory of reaction 1, if Sb=120	0·1305 "
Total loss at red heat; that is, in both stages	0·2179 "
Required by theory of reactions 1 and 2	0·2174 "

It was evident from this determination that the order of the decomposition was precisely that indicated by our reactions, although the end of the first stage was not quite so sharply marked as the end of the second; and this would naturally be expected.

As the residues obtained on distilling chloride of antimony showed, when further heated, precisely the same order of phenomena which we have just described, and when heated to redness yielded the same crystals of oxide of antimony as before, it was plain that the residue left on evaporating the chloride at a temperature not exceeding 120° was chiefly at least SbOCl; but that this when heated more intensely was converted into

$Sb_4O_5Cl_2$ before the temperature reached 280°, and finally at a red heat was converted wholly into Sb_2O_3. We therefore endeavored to determine the amount of oxichloride in one of our preparations of chloride of antimony by distilling a weighed amount from a platinum nacelle at as low a temperature as possible in a current of dry carbonic acid, and heating the residue to a temperature of about 275°. We thus obtained the following results:—

No.	Wt. of $SbCl_3$.	Residue.	% of residue $Sb_4O_5Cl_2$.
1.	6·7286	0·0212	0·315
2.	4·5150	0·0151	0·334
3.	7·9320	0·0258	0·325

In order to yield 0·146 per cent of oxygen, which would reduce the per cent of chlorine in the preparation from 47·020 to 46·608, as in the scheme on page 116, there would be required 1·155 per cent of $Sb_4O_5Cl_2$.

Although the results of the above determinations accord within a few per cent of the quantity estimated, yet it was perfectly clear during the course of the experiments that they did not at all represent the total quantity of the oxichloride present in the preparation examined. Not only was the composition of the preparation not materially altered by the slow distillation, —a fact shown by the determinations marked *e* in the table on page 47, and by which we were misled at the outset,—but also the product from our distillation yielded when distilled again apparently as much residue as before. In a word, we found the same phenomena repeated in these distillations at a low temperature which had been so noticeable when the chloride was distilled at its boiling point, and which are so strikingly illustrated by the results given on page 115. It is possible, as before suggested, that the effects might arise from a small additional absorption of water at the successive transfers which the repeated distillations involved; or, in the later experiments, from the circumstance that the very extensive apparatus employed for drying the carbonic dioxide was not completely effectual. Still, now that our attention has been called to the danger, and we had taken unusual precautions on both these points, the explanations suggested did not seem to us sufficient; and we came to the conclusion that the oxichloride must distil over with the chloride of antimony to a certain limited extent, and that it was only an excess above this definite amount which was left behind as residue. Of course, SbOCl not only is not volatile, but is at once decomposed by heat; and we do not suppose that this compound by the tension of its own vapor is carried over in distillation. It is a very dilute solution, as it were, of SbOCl in $SbCl_3$ which thus distils; and the distillation of the oxichloride may resemble the carrying over of

boracic acid by the vapor of water, and similar phenomena, the result, as it is has always appeared to us, of a feeble kind of chemical union which has been usually designated by the term "molecular combination." Such a theory would account for the remarkable constancy which we have found in the chlorine determinations of the various preparations of antimonious chloride purified by distillation. But, on account of the very great difficulty of removing all possible disturbing causes, we found it impossible to obtain a rigid experimental demonstration of our theory without much more time and labor than we could then command. We hope to return to the subject hereafter. Meanwhile, however, it was evident that we could place no reliance whatever on the results just obtained. Nevertheless, the determinations were of value on account of the contrast between these results and those of a similar series of experiments on the residues from antimonious bromide which we collect in the following table:—

No.	Wt. of $SbBr_3$.	Residue chiefly $Sb_4O_5Br_2$.	% of residue.
1.	2·8342	0·0010	0·035
2.	2·0220	0·0006	0·030
3.	4·6730	0·0010	0·021

As will be seen, this residue is less than one-tenth of that obtained from the chloride, and is practically insignificant. Evidently, then, in the determination of the atomic weight of antimony more accurate results may be expected from the analysis of the bromide than from the analysis of either the chloride or the iodide of this element. The intermediate position of the bromide renders it, in a very remarkable way, the most stable of the three compounds. It absorbs moisture far less eagerly than the chloride, and it absorbs oxygen far less readily than the iodide, and is thus in great measure protected against each of these two sources of the same impurity.

We come finally to the new analyses of antimonious chloride we had undertaken. Fortunately, some of the old preparation that had been distilled so often had been preserved. It had been boiled for a long time since the last analyses were made, and kept in the same flask used for determining its boiling point, which had stood meanwhile tightly corked in a desiccator over sulphuric acid. The solid mass in the flask was easily broken up without exposure to the air by simply heating it to the melting point, and shaking it in the flask as soon as, beginning to melt, the mass had separated from the glass. Near its melting point, chloride of antimony becomes very friable, and is thus easily reduced to coarse powder, whence probably the old alchemistic name of butter of antimony. It is also worthy of notice that neither the bromide nor the iodide

acts in this way, as we found out in more than one instance to our cost.

Thus we were readily able to prepare our material for analysis, and, by a thorough mixing of the mass, to insure that the several samples taken had a uniform composition. In regard to the antimony determination, no further details are necessary. It was conducted, as described before, with every minute precaution which experience had suggested; and we give the full details, in order to show how completely we had been able to overcome the difficulties which it at first presented, and we feel confident that there is no process of wet analysis which is capable of giving more accurate results than this.

Details of Antimony Determination.

The antimonious chloride was first transferred to a very carefully dried weighing tube, and thence to the large flask in which it was dissolved. The transfer to the weighing tube was made in a dry atmosphere, and only required two or three seconds. It is evident, however, that a slight absorption of moisture at this point is not important; for, even if it increased the apparent weight of the assay by several milligrams, it would only reduce to a barely perceptible extent the percentages of all the constituents leaving the relative values wholly unchanged. It is only when, on boiling the chloride, after such an absorption, the chlorine is driven off, that the essential change of composition results.

Weight of tube and antimonious chloride	20·9609	grams.
" " after transfer to flask	16·3920	"
" chloride analyzed	4·5689	"

The weight of the tube and chloride while on the balance pan remained invariable for a sufficient length of time to give positive assurance of the constancy of the weights. The chloride was dissolved in a saturated solution of tartaric acid containing about 15 grams of the pure acid, and then diluted with carbonic acid and water and precipitated as before described. The precipitate, having been washed and collected as before, was dried in an air bath, at about 110°.

Weight of small filter	0·0434	grams.
" porcelain crucible	101·2132	"
	101·2566	"
" crucible and precipitate	104·6762	"
" red sulphide of antimony	3·4196	"

A portion of the dried precipitate dissolved in hydrochloric acid gave no residue. The rest was then transferred to a pla-

tinum nacelle, and heated, as has been described, in a current of dry carbonic dioxide gas. No sublimate was formed, and only a very slight empyreumatic odor could be perceived.

Weight of platinum nacelle	6·2493	grams.
" nacelle and dried precipitate	9·5273	"
" portion taken	3·2780	"
" nacelle and precipitate after heating to 285° for over half an hour	9·5234	"
Loss of weight of portion taken	0·0039	"
Corresponding loss for whole precipitate	0·0041	"
Weight of red sulphide as above	3·4196	"
" gray sulphide	3·4155	"

The carbonaceous residue left on dissolving this whole amount of gray sulphide in hydrochloric acid was barely perceptible. It was collected, however, as usual, on a weighed paper disk, and estimated.

Weight of small paper filter	0·0198	grams.
" same with residue	0·0212	"
" residue	0·0014	"
Calculated for whole precipitate	0·0015	"
Weight of gray sulphide as above	3·4155	"
Total weight of gray sulphide	3·4140	"
Corresponding weight of antimony assumed to be $\frac{2}{3}$ of the sulphide	2·4386	"
Per cent of antimony in the antimonious chloride under examination	53·374	"

It will be noticed that this result is practically identical with the mean of the previous determinations, which, as will be seen by reference to the table on page 48, was 53·401; and, by reviewing the facts stated in that connection, it will be perceived that this agreement is in itself alone a strong confirmation of the conclusion which we deduced from our first experiments on the synthesis of the gray sulphide of antimony,—that of the two values of the atomic weight of antimony in question, the lower is the more exact.

Coming next to the chlorine determinations, we noticed, for the first time, an effect which, under certain circumstances, may have an important influence on the accuracy of this well-known process, as employed in the analysis of chloride of antimony. In a precipitate of argentic chloride that had been deposited

from an unusually concentrated solution of antimonious chloride in tartaric acid, and had stood over night, our attention was called to some crystalline grains, which, on examination, proved to be a compound of tartaric acid, antimony and silver. We soon found that this product could be readily obtained by concentrating the filtrate from the precipitate of argentic chloride, and adding to it, while still warm, an excess of argentic nitrate. On cooling, the new crystals form in abundance. They have not yet been measured, but under the microscope they have the general aspect of right rhombic plates or prisms, with hemihedral modifications,—a general form which is so characteristic of the tartrates, and which we ourselves have previously studied in our crystallographic determinations of the tartrates of rubidium and cæsium.* We obtained for the amount of silver in the crystals, as a mean of three analyses, 26·30 per cent. The compound $Ag, SbO.H_2=O_4=(C_4H_2O_2)$. H_2O would require 26·34 per cent. The crystals may therefore be regarded as tartar emetic, in which the potassium has been replaced by silver; and they resemble the crystals of this well-known salt in general form. They are evidently the same substance obtained by Wallquist† by precipitating nitrate of silver with tartar emetic, and analyzed both by him and by Dumas and Piria. These chemists obtained respectively 27·31 and 28·05 per cent. of oxide of silver, which corresponds with the result given above as closely as could be expected; but they appear to have prepared the substance only in an amorphous condition. At least, in the description quoted, no mention is made of any crystalline form.

These crystals of argento-antimonious tartrate are apparently not acted upon in the least by cold water, and only slightly by boiling water; and finding this very insoluble material mixed with the precipitated chloride of silver, under the conditions stated, we were led to fear that it might be occluded to some extent by this precipitate, even when formed in much more dilute solutions of antimony and tartaric acid. The phenomenon was very similar to that we had already studied in the occlusion of the oxichloride by the sulphide of antimony; and there was reason to fear that, as in the previous case, an occlusion of this double tartrate might result, even when the substance would not otherwise be precipitated. How far such an action could have vitiated our previous results, it was, of course, now impossible to determine; but, as we previously stated, we had always taken great care not to add more than the slightest possible excess of argentic nitrate, and this was especially true in our more recent determinations. Now, however, we were

* Am. Jour. of Science and Arts, II, xxxvii, 70.
† Gmelin Handbook, Cavendish Edition, x, 326.

on our guard, and in the following determinations very great pains were taken to add just the requisite amount of the silver salt, and the argentic chloride was subsequently examined for traces of any such occlusion. But, excepting this close attention to well-known precautions, the determinations were made in the same way as before.

ANALYSIS OF ANTIMONIOUS CHLORIDE.

No.	Wt. of $SbCl_3$.	Wt. of AgCl.	% of Chlorine.
1.	2·2220	4·1682	46·407
2.	1·9458	3·6512	46·420
	Mean value		46·413

Bringing now the results together,—estimating the amount of oxygen by difference, as is usual in chemical analysis, and calculating what would be the composition of a preparation of antimonious chloride in which $\frac{2}{1}\frac{1}{0}\frac{3}{0}\frac{}{0}$ of a per cent. of oxygen had replaced an equivalent amount of chlorine, assuming, of course, Sb = 120 and Cl = 35·5,—we obtain the following very striking accordance:—

	Analysis.	Theory. Sb = 120, Cl = 35·5
Chlorine	46·413	46·418
Oxygen	·213	·213
Antimony	53·374	53·369
	100·000	100·000

The general conclusions, then, which we deduce as the results of this investigation, are—

First, that the value of the atomic weight of antimony found by Schneider in 1856—Sb=120·3—must be accurate within a few tenths of a unit, but that the most probable value of this constant, as deduced from our experiments, is Sb=120, when S=32.

Secondly, that the apparent disagreement with this result, presented by the partial analyses of antimonious chloride, is *probably* due to the constant presence of oxichloride in the preparations of this compound.

The investigation from the first has been a study of constant errors; and those who have followed us through the details will certainly allow that the opinions expressed at the beginning of this paper (on page 42) were not hastily conceived, even if they do not fully agree with our conclusions. In the attempts to correct or balance such errors, we have found at once the chief difficulties and interest of our work, and the secondary results thus reached seem to us the most important fruit of the whole investigation. Seeing, then, the sources of constant error we have discovered, and knowing that there are others

whose influence we have been able to trace, although we have not been able to define them as clearly as we could desire, it would be presumptuous in us to express too great confidence either in the correctness of our theories or even in the conclusiveness of our experimental results. Of this, however, we feel assured, that more trustworthy results cannot be expected from a repetition of the same processes until a more complete and accurate knowledge has been acquired of the substances employed. We have therefore proposed to ourselves a more thorough investigation of the haloid compounds of antimony, and the first results of this investigation we shall shortly publish. After the requisite data have been thus collected, we hope to return to the old problem with such definite knowledge of the relations involved as will enable us to obtain at once more sharp and decisive results than are now possible.

During the course of this investigation, we have been successively aided in the experimental work by Dr. F. A. Gooch, Mr. C. Richardson and Mr. W. H. Melville, at the time students in this laboratory; and without their assistance we could not have accomplished the great amount of labor it involved.

Harvard College Laboratory, June 12th, 1877.

Art. XVI.—*Descriptions of two new species of Primordial Fossils;* by S. W. Ford.

Protocyathus rarus, gen. et sp. nov.

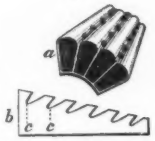
The fossil form for which the above generic and specific names are proposed belongs to the *Archæocyathus* group, and finds its nearest analogue in *Archæocyathellus* of the writer from the same locality and geological horizon.* The only specimen at present known to me is but 0·22 of an inch in length and has a width of only 0·16 of an inch at the larger extremity. The general form is that of a minute cone with the apex broken off. The width at the smaller extremity is 0·12 of an inch, and of this fully one-third is occupied by the cup. The cup itself is filled with light colored limestone, rendering it easily distinguishable from the interseptal areas, which are filled with a darker colored material. These latter appear to have been twenty-eight in number. The radiating septa may be observed in two or three places, and are seen to be thin and delicate. The outer wall has been almost wholly removed and the portions of it that remain are much weathered. The material presented for study consists, therefore, of the solid moulds of the interseptal spaces, the cup filled with limestone, a small number of the septa, a

* This Journal, March, 1873.

transverse section of the inner wall and the impression of a considerable portion of the outer wall. The latter shows that the external surface when perfect was longitudinally furrowed as in *Archæocyathellus*. In that genus, however, so far as known, there are two rows of pores along each of the furrows, one on either side of the septa; whereas, in the present genus there appears to have been but one, and that placed directly on the line of the septa. The evidence of this consists of rudely circular holes placed at regular intervals along the middle of each furrow in the cast. These appear to me to argue the existence of funnel-like projections inward of the outer wall at the place of the openings. That they mark the position of orifices leading into the interior appears to me in the highest degree probable. Their position is, however, so remarkable, that I was for a long time unable to understand the meaning of them.

On one side of the specimen there are a small number of the interseptal moulds that project beyond the others and one of these shows one of its lateral faces for a considerable distance lengthwise, and also nearly down to the outer surface of the inner wall. An examination of this face shows that the cavities observed along the furrows extend but a short distance inward, and that the septa arched around the funnel-like projections which they represent from below, striking the outer wall only at the intervening spaces (the spaces between the dots in the figure). It is further shown that these cavities are directed slightly upward or toward the aperture of the cup. These characters serve to distinguish the form at once from *Archæocyathellus* in which the septa meet the outer wall uninterruptedly.

1.*



If I am right in viewing the cavities along the furrows in the cast as indicating the presence of external orifices at these points, then it follows that these orifices were doubtless functionally equivalent to the double row of orifices along the furrows of the outer wall of *Archæocyathellus*. In proof of this it may be remarked that the size of the cavities indicates that the orifices were proportionally considerably larger than those or the only known species of *Archæocyathellus* (*A. Rensselaericus*), while their position is such as to present no obstacle in the way of regarding them as having communicated simultaneously with two of the interseptal spaces.

The fossils of this group have, in their septate structure

* Fig. 1*a*.—A few of the interseptal moulds of *Protocyathus rarus* enlarged to show the position of the supposed external orifices; *b*, enlarged outline of a lateral face of one of the moulds designed to show the direction of the cavities *c*, *c*.

much the appearance of corals; but the peculiar poriferous structure known to characterize a good typical species, the uniform presence of a large walled central cavity, and the existence in one species (*Archæocyathus Minganensis*) of branched spiculæ would seem to ally them more nearly to the sponges. By the late lamented Mr. Billings, to whom we owe our first knowledge of these singular forms, and who has discussed their affinities at considerable length, they were classed provisionally with the sponges; but in conclusion he remarks that "The resemblance between the whole structure and that of the paleozoic corals seems also to show that in the Lower Silurian seas forms existed combining the characters of the Protozoa and the Cœlenterata."* The existence of such forms in our older deposits is a matter of much interest, and it is to be hoped that continued researches will add still further to our knowledge of them.

This species occurs in conglomerate-limestone of the Lower Potsdam group at Troy, N. Y.

Solenopleura nana, sp. nov.—Of this species I have a number of specimens of the head, but they are all more or less imperfect. The largest and best preserved specimen consists of a nearly perfect glabella and the greater portion of the fixed cheeks, and is but two lines in length. The glabella is nearly four-fifths the total length of the head and is especially characterized by its great relief. It is obtusely conical, slightly widest behind, and is well defined all around by the dorsal furrows. In a specimen two lines in length its highest point is nearly one and one-half lines above the base of the fixed cheeks. It is marked on either side by two or three faint furrows. The fixed cheeks are notably convex, but their relief does not exceed one-third of that of the glabella. The eyes are situated slightly in advance of the mid-length and are connected with the front of the glabella by an obscure ocular fillet. The distance from the eye to the glabella is nearly equal to the width of the glabella at the mid length. The front margin is narrow and is bounded by a feebly convex rim, inside of which there is a narrow furrow which gradually deepens on either side of the median line in passing outward. Between this furrow and the glabella there is a somewhat angular ridge which widens in passing outward to the sutures.

The course of the facial suture is nearly the same with that of *Solenopleura brachymetopa* of Angelin (Palæontologia Scandinavica, Pl. xix, fig. 1), but is directed slightly more inward in front of the eye. The neck-furrow is continuous all across. The exact form of the neck-segment cannot be made out, owing to the damaged condition of all of the specimens at this point. It is seen, however, to be less elevated than in the ma-

* Pal. Foss., vol. i, p. 357.

jority of the species, not rising above the surface of the fixed cheeks. The entire surface is covered with a fine regular granulation.

This species is the second one of the genus, so far as I am aware, that has been described from American rocks, the first one having been obtained from strata of the Acadian epoch in Newfoundland and described by Mr. Billings. It occurs in both even-bedded and conglomerate limestone of the Lower Potsdam group at Troy, N. Y., associated with *Olenellus*, *Conocoryphe* and *Microdiscus*. It is principally interesting on account of its affording another generic link between the already closely related faunæ of the Acadian and Lower Potsdam.

New York, Oct. 13th, 1877.

ART. XVII.—*Note on Lingulella cælata;* by S. W. FORD.

THE above mentioned species, occurring in the Troy Primordial, has hitherto been set down by me as an *Obolella*, but the evidence now in hand shows that it should be referred to the genus *Lingulella* of Salter. The following are the principal characters:

The ventral valve is somewhat elongate-ovate, with the beak pointed, slightly elevated and conspicuously channeled for the passage of the pedicle. The convexity is moderate and nearly uniform. On the inside there are two prominent, elongate, curved scars, one on either side of the median line, with their concavities directed outward. These recall by their form and position the large lateral scars of the ventral valve of certain species of *Obolella* (e. g., *O. chromatica.*) The other impressions of this valve have not been made out.

The dorsal valve is more rotund than the ventral and has the beak much depressed. The convexity increases with increasing age, and in adult specimens is such as to sometimes give the valve a semi-globose appearance. A shallow depression extends in all the specimens from the beak to the front margin, but in fully grown forms it is often inconspicuous. On the inside there are four prominent ridges. Of these the more central two commence close to the median line a short distance in front of the beak and extend into the forward third of the shell slightly diverging throughout, while the lateral pair take their rise close to the beak and reach to points a little in advance of the mid-length. There is also a short slender ridge directly beneath the beak on the median line. The central portion of the valve in the upper half is slightly excavated. The description of the interior of this valve has been mainly drawn up from an excellent natural internal mould.

The surface of both valves is ornamented with moderately conspicuous radiating and concentric lines, the latter irregularly grouped, and covering the whole a fine papillose network, the points of which are arranged in concentric series, those of one series alternating with those of the next, and so on, as first pointed out by Professor Hall in his description of the dorsal valve (Pal. N. Y., vol. i, p. 290, pl. 79, fig. 9). The effect of this style of ornamentation is very beautiful; and when, as is usually the case, the shells have a dark, polished aspect, with a setting of light-colored limestone, few handsomer fossil objects can be named. The shell is thick and of a finely lamellar structure. The usual length of the ventral valve is about three and one-half lines.

This is one of the best marked fossils of the Troy Primordial and may be easily identified by means of very small fragments.

The species known as *Obolella crassa* of the Troy beds may also be briefly noticed in this connection. It includes the species already widely known under the name of *O. desquamata* from the same locality, this latter, as may be shown, having been founded upon the dorsal valve of the former. The ventral valve is always more acutely pointed at the beak than the dorsal, but beyond this feature there is nothing, so far as I have been able to discover, by which they may be distinguished from each other externally. The surface of each, when perfect, is both radiately and concentrically striated. As a rule, however, the imbricating edges of the successive layers of growth are the only markings visible.

Of the interior of the ventral valve an excellent figure was given by Mr. Billings on page 355 of this Journal for May, 1872; but the interior markings of the dorsal valve have nowhere, to my knowledge, yet been accurately shown. The scars are nearly the same with those of the dorsal valve of *O. chromatica*,* but the smaller pair close to the beak are here, in the majority of cases, distinctly connected with the larger pair directly beneath them; while the central pair, instead of running parallel with each other throughout, diverge at the mid-length of the valve, and extend onward in slender falcate forms into the anterior fourth of the shell. Their parallel portions are, however, the only parts usually seen, and it was only after collecting the species for a number of years that I obtained evidence that what had come to be looked upon as wholes were, in reality, only parts of much more extensive impressions.

The species of Brachiopoda at present known to me from the Troy Primordial are the following: *Obolella crassa*, *O. gem-*

* "On the structure of *Obolella chromatica*," by E. Billings, F.G.S. This Journal, March, 1876.

*ma** (Billings sp., or a species which I am unable to distinguish from this form by any good characters), *O. nitida*, *Lingulella cælata*, and a small species of *Orthis* yet undescribed. This latter species is about one-third smaller than *Orthis Billingsi* of the Acadian group† which it otherwise much resembles, except that the ribs do not dichotomize as in that species. None of the specimens yet obtained are sufficiently perfect to admit of a full description.

New York, Oct. 31st, 1877.

Art. XVIII.—*Note on the Development of Olenellus asaphoides;* by S. W. Ford.

Since the publication of my paper giving an account of the metamorphoses of this remarkable trilobite,‡ I have obtained at Troy a number of specimens further illustrating and confirming the fact of the metamorphoses. Among the more important of these is a beautifully preserved cephalic shield showing the manner in which the appendages that I have called the inter-ocular spines were finally lost. As this specimen supplies one of the most important links in the demonstration and fully confirms what was inferred from the structure of previously known specimens representing other phases of the development, the more interesting features which it presents may be briefly noticed at this time.

The specimen in question is almost exactly intermediate between the forms represented by figures 3 and 4 of my former paper. Excepting the neck-furrow and the second pair of furrows in advance, none of the glabellar furrows reach the median line; while the inter-ocular spines, still further reduced in size, are seen to be entirely cut off from the swollen spaces between the eye-lobes and glabella, by the furrows immediately within the eye-lobes extending completely across them, and uniting with the marginal furrows. There can be scarcely a doubt but that the next moult would result in a head destitute of these appendages, as in fact, we find the forms next in order of increasing size to be. The dwarfed proportions of the appendages lead also to the conclusion that they are examples of atrophied organs, as has likewise been suggested to me by M. Barrande. I know of no instance of this—the suppression of spinous

* This Journal, May, 1872, p. 355.

† Acadian Geol., Dawson, 1868, p. 644. Also Dana, Man. of Geol., 1874, p, 174, fig. 250.

‡ This Journal, April, 1877.

appendages beyond the contour — in any other species of trilobite.

The surface of the cheeks in the specimen under notice is beautifully ornamented with fine, waved, radiating lines as in the adult.

I may also add, that several of the different stages of growth observed are shown to be represented by two distinct forms, respectively a long and a broad form. The same thing has been stated by Barrande for a large number of Bohemian species. Some of the earlier forms were very diminutive, the smallest specimen now in my possession being one-third smaller than the smallest example yet illustrated. I have also observed one specimen the width of which did not exceed $\frac{1}{5}$th of an inch.*

New York, December 10th, 1877.

* Neither the small trilobite recently described by G. Linnarsson under the name of *Paradoxides aculeatus* (Transactions of the Geol. Soc. of Stockholm for 1877, p. 359) nor the species previously described as *Paradoxides Kjerulfi* by J. G. O. Linnarsson furnish, in my opinion, any proof of the metamorphoses of Trilobites, or suggest, in their structure, a generic identity with *Olenellus asaphoides*. Moreover, I greatly question whether the two Swedish species above mentioned are truly congeneric, and am strongly disposed to believe that the former will yet turn out to be a genuine *Hydrocephalus*. Mr. Linnarsson notes the close agreement with *Hydrocephalus*, but makes the separation mainly on the strength of his suspicion that Barrande is in error in his diagnosis of the genus. I believe the direction of the facial suture in *P. aculeatus* to be utterly fatal to Mr. Linnarsson's views. There is nothing, to my mind, in the structure of the specimens figured, to lead to a comparison of the posterior spines of *P. aculeatus* wtth the smaller pair of *P. Kjerulfi*. In regard to Mr. Linnarsson's somewhat extended, but, as it appears to me, too assured criticism of my work in the publication referred to, I may say that I see no reason to change any of the statements contained in my former paper. Whatever Linnarsson's *Paradoxides aculeatus* may hereafter prove to be, I can confidently assert that *Olenellus asaphoides* is not a true *Paradoxides*. When the development of some species of *Paradoxides* shall have been made out (if, indeed, any of the species ever sustained one) we shall probably be able to satisfy ourselves still more fully upon this point. The smaller spines of the posterior margin of *P. Kjerulfi* suggest a comparison with the inter-ocular spines of *Olenellus asaphoides*, and G. Linnarsson was the first to perceive this; but at present I think it extremely doubtful whether they can be properly regarded as homologous. This will become the more apparent if we compare the figures of *P. Kjerulfi* with my figures 4 and 5. Be this as it may, the relations of the two genera are manifestly very close, and the Swedish beds fortunately promise to contribute largely toward working them out to completeness. Mr. Linnarsson considers *O. asaphoides* to be a true *Paradoxides* and his *P. aculeatus*, one example of which he figures, an embryonic form of the same genus; but finding that the forms of my young specimens do not square with his figure he thinks my account most probably deeply in fault. Were his conclusions and conjectures supported by a better array of facts drawn from his own special field of observation they would possess greater fitness and value.

ART. XIX.—*On Schweitzer's "New Acid Ammonium Sulphates;"* by S. W. JOHNSON and R. H. CHITTENDEN *Contributions from the Sheffield Laboratory of Yale College.* No. LI.

DR. Paul Schweitzer, in a paper "On some New Acid Ammonium Sulphates, read before the American Chemical Society, July 6, 1876,* has given the results of some partial analyses of residues remaining after subjecting ammonium sulphate to several degrees of ignition, and has inferred: 1. That exposure to a heat a little higher than that of the boiling point of mercury converts ammonium sulphate into ammonium bisulphate with loss of one-half of its ammonia. 2. That a temperature somewhat below incipient redness occasions further loss of ammonia and sulphuric acid and leaves a salt of the formula $(NH_4)_2H_4(SO_4)_3$. 3. That probably an intermediate salt is formed having the formula $(NH_4)_4H_2(SO_4)_3$.

These conclusions are based on the fact that the residues of ignition at the temperatures named yield such percentages of SO_3 as the above formulæ require. We have repeated most of Dr. Schweitzer's experiments, and so far as we have gone, have fully verified his observations. The formulæ which he deduces from his estimations of SO_3 are, however, inconsistent with the usually received atom-fixing powers of the elements involved, and we have made further investigation of the substances to which he has called attention, in order to ascertain whether they are really exceptions to the laws of valence, and therefore possibly serviceable means of enlarging our generalizations, or have a composition different from that which Dr. Schweitzer has inferred.

Ammonium sulphate† heated for several hours somewhat higher than the boiling point of mercury at first fused without effervescence to a thick pasty mass, as Dr. Schweitzer has stated, but afterwards became fluid. When the fumes no longer had an alkaline reaction, and "ammonia" ceased to be given off, the residue yielded nearly the same percentage of SO_3 obtained by Dr. Schweitzer. He found 69·17 per cent and after fifteen minutes further ignition 69·49 per cent. We found 69·60 per cent, and after further heating at the same temperature 70·03 per cent of SO_3. The bisulphate requires 69·56 per cent. We notice, however, that the per cent of SO_3, which in this case admits of very exact estimation, increases as the ignition is prolonged. A second sample gave in the

* American Chemist, Aug., 1876, p. 42.

† Made from sulphuric acid and commercial ammonium carbonate, and analyzed with following results:

	Found.	Calculated.
SO_3	60·70	60·60
$(NH_4)_2O$	39·28	39·39

first analysis 68·95 per cent and after a second ignition 69·41 per cent SO_3. To make the analyses more complete, ammonia was estimated in both samples by distillation with sodium hydroxide, and in the second sample hydrogen was determined by combustion with lead chromate and metallic copper and found to be 4·67 per cent.

On the basis of the ammonia estimation we have the following statement.

	Found. 1.	Found. 2.	Calculated for bisulphate.
SO_3	70·08	69·41	69·56
NH_3	17·00	17·08	14·78
Difference	12·92	13·51	H_2O 15·66
	100·00	100·00	100·00

The ultimate composition, reckoning oxygen by difference, is—

	Found. 1.	Found. 2.	Calculated for bisulphate.
S	28·01	27·76	27·82
O		53·49	55·65
N	14·01	14·08	12·17
H		4·67	4·34
		100·00	99·98

On dissolving in water the solution has an acid reaction, addition of strong alcohol to the saturated solution throws down a crystalline precipitate which is normal ammonium sulphate and yielded in the results of two successive determinations 25·48 per cent and 25·65 per cent of NH_3. Theory requires 25·76 per cent. The alcoholic mother liquor from these determinations gave on evaporation a small crystalline residue, and a few drops of a strongly acid liquid. The absence of any amide or amic acid was shown by the prompt and complete precipitation of all the sulphur by barium salts in cold-prepared solutions. These results would indicate that the supposed bisulphate is a mixture of normal sulphate with bisulphate or pyrosulphate.

The analyzed substance compared with the salts just named in respect to atomic ratios gives the following results, eight atoms of sulphur being assumed in each case for convenience.

Normal sulphate.	Bisulphate.	Pyrosulphate.	Scweitzer's 1st substance.
S= 8	8	8	8
N=16	8	12	9·3
H=64	40	26	43
O=32	32	24	30·8

Inspection of the above figures makes evident that the analyzed substance must contain besides normal sulphate, a cer-

tain proportion of bisulphate in order to bring down the nitrogen below twelve and also some pyrosulphate to reduce the oxygen below thirty-two.

Calculation shows, in fact, that the substance is a mixture of nearly one molecule of pyrosulphate $(NH_4)_2S_2O_7$, one molecule of sulphate, $(NH_4)_2SO_4$, and three molecules of bisulphate $3(NH_4HSO_4)$. Such a mixture would have the following empirical expression: $S_6O_{23}N_7H_{31}$ and its centesimal composition compares closely with our analyses.

	Calculated.	Found.	
		1.	2.
6S	27·86	28·01	27·76
23O	53·41		53·49
7N	14·22	14·01	14·08
31H	4·50		4·67
	99·99		100·00

Our examination of the so-called "biammonium tetrahydrogen sulphate" obtained by subjecting ammonium sulphate to near incipient redness demonstrates that it also is a mixture. The facts given by Professor Schweitzer agree substantially with those observed by us.

He found in the residue after two successive heatings 72·52 and 72·95 per cent SO_3. We found 72·54 and 72·75, and in another sample 71·91 and 72·04 per cent. Our complete analyses do not agree with Professor Schweitzer's formulæ, but indicate that the substance is very nearly a mixture of two molecules of ammonium bisulphate $(NH_4)HSO_4$ with one molecule of pyrosulphate $(NH_4)_2S_2O_7$. Such a mixture is represented empirically by $S_4O_{15}N_4H_{18}$. The percentages required by it and those found in our analyses are subjoined.

		Found.		
	Calculated.	1.	2.	3.
4S	28·96	29·05	29·01	28·81
15O	54·29	53·85	53·78	
4N	12·67	12·94	12·94	12·96
18H	4·07	4·16	4·27	

This mixture dissolved readily in a small proportion of water, giving a solution acid to test papers and to the taste. The concentrated solution stood for two weeks without crystallizing. On adding a little alcohol (93 per cent) and agitating, oily-appearing drops separated, which shortly united to a heavy layer at the bottom of the vessel, and very soon large prisms appeared in it. More absolute alcohol was added, and after twenty-four hours standing a fine crop of crystals was obtained. These crystals A were rinsed with alcohol and dried at 100° C. To the filtrate absolute alcohol was added as long as a precipitate separated. On standing an abundant deposit of acicular

crystals formed. These B were separated and dried at 100 C. These crystals proved to be normal ammonium sulphate.

They yielded by analysis—

	A	B	Calculated.
$(NH_4)_2O$	39·33	39·46	39·39

The alcoholic mother-liquor was evaporated on the water-bath and left a small fluid residue, which on cooling deposited a few crystals, apparently of normal sulphate. The few drops of liquid remaining were intensely acid and had all the characters of sulphuric acid discolored by organic matters. Treatment with aqueous alcohol thus resolves both the bisulphate (two molecules) and the pyrosulphate into normal sulphate and sulphuric acid, or in part probably into sulphethylic acid.

In the first stage of the decomposition of ammonium sulphate at a temperature "a little higher than the boiling point of mercury" the vapors are alkaline. The chemical change would appear to involve six molecules of the sulphate, which lose five molecules of ammonia gas and one molecule of water vapor, leaving as solid residue a mixture of one molecule of unchanged sulphate, one of pyrosulphate and three of bisulphate.

$$6[(NH_4)_2SO_4]-[5NH_3+H_2O]=3[NH_4HSO_4]+(NH_4)_2S_2O_7+(NH_4)_2SO_4$$

In these changes two molecules of sulphate yield one molecule of pyrosulphate with loss of one water- and two ammonia-molecules,* while three molecules of sulphate yield, each, a molecule of bisulphate, with loss of a molecule of ammonia,† and the sixth molecule of sulphate comes out unaltered.

In the second stage of heating (near incipient redness) the fumes are at first alkaline, but shortly become acid, and continued so as long as that temperature is maintained.

The changes are empirically expressed as follows:

$$S_6O_{23}N_7H_{31}-[2SO_3+2H_2O+3NH_3]=S_4O_{15}N_4H_{18},$$

or rationally—

$$[3(NH_4HSO_4)+(NH_4)_2S_2O_7+(NH_4)_2SO_4]-[2SO_3+2H_2O+3NH_3]=2(NH_4HSO_4)+(NH_4)_2S_2O_7.$$

The rise of temperature from 350° to 520° C. appears not to alter the pyrosulphate and bisulphate, but the chemical change seems to result from a molecule of sulphate reacting on a molecule of bisulphate, whereby both are decomposed, thus—

$$NH_4HSO_4+(NH_4)_2SO_4=2SO_3+2H_2O+3NH_3.$$

It would be interesting to study the reactions at other temperatures.

* $SO_2<\begin{matrix}ONH_4\\ONH_4\end{matrix}$ $SO_2<\begin{matrix}ONH_4\\ONH_4\end{matrix}$ $-[H_2O+2NH_3]=$ SO_2/ONH_4 $SO_2>O$ $SO_2<\begin{matrix}ONH_4\\ONH_4\end{matrix}$ ONH_4.

† $3SO_2<\begin{matrix}ONH_4\\ONH_4\end{matrix}-3NH_3=3SO_2<\begin{matrix}OH\\ONH_4\end{matrix}$.

ART. XX.—*The Poplars of North America;* by SERENO WATSON.

THE following incomplete synopsis of the species of *Populus* is based upon the material in several of our principal herbaria, and is published for the purpose of drawing the attention of botanists during the coming season to this still very imperfectly known genus. Flowers and fruit even of the common species are too rare in collections, and are much needed for their satisfactory definition.

§ 1. Styles two, with two or three narrow or filiform lobes: capsules small, thin, oblong-conical, two-valved: seeds very small: leaves ovate.

* Petioles flattened: bracts silky: stamens six to twenty.

1. *P. tremuloides* Michx. 2. *P. grandidentata* Michx.

** Petioles terete: bracts not silky: stamens twelve to sixty.

3. *P. heterophylla* L.

§ 2. Styles two to four, with dilated lobes: capsules large, often thick, subglobose to ovate-oblong, two to four-valved: bracts mostly glabrous.

* Leaves cordate or ovate to lanceolate, crenate; petioles terete: stamens twelve to thirty: seed a line long.

† Capsule glabrous, two-valved.

4. *P. balsamifera* L. Leaves whiter beneath, ovate-lanceolate, acuminate, glabrous; petioles one-half to two inches long, at first puberulent: rhachis of aments (pistillate two and one-half to five inches long) pubescent: disk one or two lines broad: stamens twenty to thirty: capsules ovate (three lines long), on very short pedicels.—Var. *candicans* Gray. Leaves broader and cordate; the petioles often somewhat hairy.—Var. (?) *Californica.* Leaves ovate to oblong-lanceolate, acute, usually rounded at base, somewhat pubescent beneath, mostly on short petioles (half inch long or less).—The typical form northward to the Arctic Ocean; var. *candicans* west to Colorado and Idaho; the last variety on the Pacific side from Los Angeles to Oregon, but known only from the foliage.

5. *P. angustifolia* James. Leaves not whiter beneath, rhombic-ovate to narrowly lanceolate, mostly cuneate at base, often small; petioles one-half inch long or less (rarely one inch): rhachis glabrous (pistillate two inches long, rarely two or four): disk a line broad or less: stamens twelve: capsules ovate, smaller, on very short pedicels.—Colorado and New Mexico to Central Arizona and Nevada, and Washington Territory. Two forms are spoken of, the "Yellow Cottonwood," making fair lumber, and the "Black Cottonwood," common

and extensively planted in Utah, but the wood considered worthless.

†† Capsule tomentose, three-valved.

6. *P. trichocarpa* Torr. and Gray. Leaves broadly ovate, acuminate, cordate, often whiter beneath with age, puberulent when young; petioles one or two inches long: rhachis pubescent (pistillate two to six inches long): disk very broad: stamens twenty to thirty: capsule subglobose, nearly glabrous.—Var. *cupulata.* Disk somewhat herbaceous, very large and campanulate, twice longer than the ovary, pubescent: bracts villous: pedicels a line or two long.—S. California to W. Nevada and British Columbia.

** Leaves deltoid, sinuate-crenate; petioles flattened: stamens sixty or more: seed one and one-half or two lines long: capsule three or four-valved: rhachis and disk glabrous.

7. *P. monilifera* Ait. Leaves with numerous serratures and narrow very acute acumination, broadly truncate-deltoid, sometimes ovate, rarely cordate; petioles two to four inches long: ament usually long (two to seven inches): disk rarely two lines broad: capsules rather thin, oblong-ovate, four or five lines long, on slender pedicels one to five lines long.—New England to Florida, Louisiana, and the base of the Rocky Mountains in Colorado and Wyoming. Most flowering and fruiting specimens seen from east of the Mississippi have four, rarely three, distinct styles and a four-valved capsule; a single specimen from the Agricultural Grounds, Washington (cultivated as *P. angulata*), has the styles united and bearing a peltate stigma, and the capsule three-valved. The more western specimens have all three distinct styles and a three-valved capsule. There are no apparent differences otherwise, and it remains to be seen whether these forms can be specifically separated.

8. *P. Fremonti* Watson. Leaves with few serratures (four to twelve on each side), broadly deltoid with a broad acute apex and usually somewhat reniform or cordate; petioles one to two and one-half inches long, often pubescent (as the branchlets) with short spreading hairs: staminate disk three or four lines broad, and pedicels eight to ten lines long: pistillate aments three or four inches long: disk three lines broad: capsules ovate, thick-coriaceous, three-valved, on stout pedicels two lines long or less.—Var. (?) *Wislizeni.* Leaves sharply acuminate, truncate or slightly cuneate at base: staminate disk less dilated, and the pedicels shorter: pistillate aments very slender (two to six inches long), the disk two or three lines broad, and the somewhat angled capsules three- or usually four-valved, on slender pedicels two to eight lines long.—The typical form from N. California to S. Utah; the variety from S. California to the Rio Grande.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Liquefaction of Oxygen;** by M. Raoul Pictet.—The object which I have had in view for more than three years is to demonstrate experimentally that molecular *cohesion* is a general property of bodies, to which there is no exception.

If the permanent gases are not capable of liquefying, we must conclude that their constituent particles do not attract each other, and thus do not conform to this law.

Thus, to cause experimentally the molecules of a gas to approach each other as much as possible, certain indispensable conditions are necessary, which may be expressed thus:—

(1.) To have the gas absolutely pure, with no trace of foreign gas.
(2.) To be able to obtain extremely energetic pressures.
(3.) To obtain intense cold, and to subtract heat at these low temperatures.
(4.) To utilise a large surface for condensation at these low temperatures.
(5.) To be able to utilise the rapid expansion of the gas from extreme condensation to the atmosphere pressure—an expansion which, added to the preceding means, will *compel* liquefaction.

Having fulfilled these five conditions, we may formulate the following alternative:—

When a gas is compressed to 500 or 600 atmospheres, and kept at a temperature of $-100°$ or $-140°$, and it is allowed to expand to the atmospheric pressure, one of two things takes place:—

Either the gas, obeying the force of cohesion, liquefies, and yields its heat of condensation to the portion of gas which expands and loses itself in the gaseous form; or, on the hypothesis that cohesion is not a general law, the gas must pass to the absolute zero and become inert,—that is to say, an impalpable powder.

The work done by expansion will not be possible, and the loss of heat will be absolute.

Struck with the truth of this alternative, which is rendered certain by thermo-dynamic equations based on accurate data, I have sought to produce a mechanical arrangement which should entirely satisfy these different conditions, and I have chosen the complicated apparatus of which the following is a brief description:—

I take two pumps, P_3 and P_4, for exhaustion and compression, such as are used industrially in my ice-making apparatus. I couple these pumps in such a way that the exhaustion of one corresponds to the compression of the other. The exhaustion of the first communicates with a tube (R) of 1·1 metres long and 12·5

* The *liquefaction of oxygen* is so important a scientific achievement that we have much pleasures in laying before our readers the following detailed account of the means employed and diagrams of the apparatus used, which have been communicated to us by M. Pictet himself.—Ed. Chemical News.

centimeters in diameter, and filled with liquid sulphurous acid. Under the influence of a good vacuum the temperature of this liquid rapidly sinks to $-65°$, and even to $-73°$, the extreme limit attained.

Through this tube of sulphurous acid passes a second smaller tube (S), of six centimeters diameter, and the same length as the envelope. These two tubes are closed by a common base.

In the central tube is retained compressed carbonic acid produced by the reaction of hydrochloric acid on Carrara marble. This gas, being dried, is stored in an oil gasometer (G) of one cubic meter capacity.

At a pressure of from four to six atmospheres the carbonic acid easily liquefies under these circumstances. The resulting liquid is led into a long copper tube (B), four meters in length and four centimeters in diameter.

Two pumps, P_1 and P_2, coupled together like the first, exhaust carbonic acid either from the gasometer (G) or from the long tube (B) full of liquid carbonic acid.

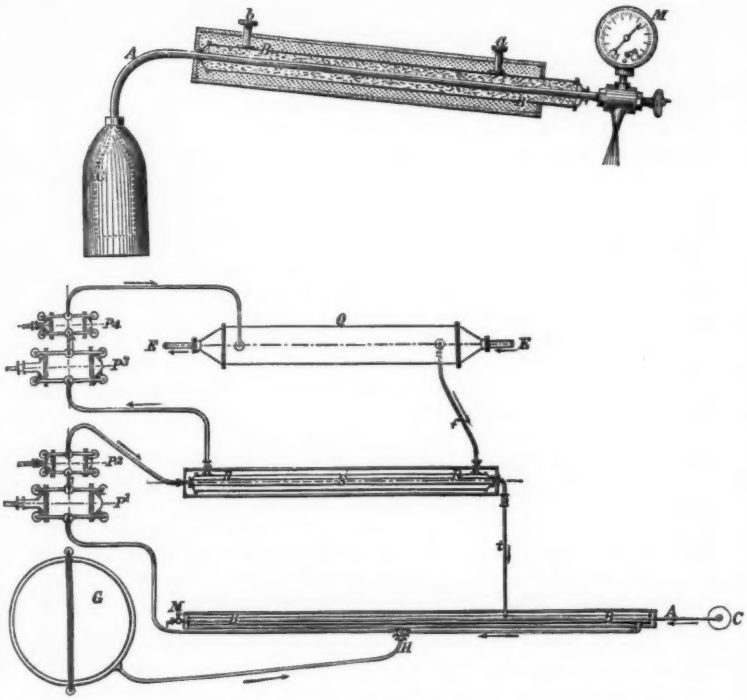
The ingress to these pumps is governed by a three-way tap, H. A screw valve cuts off at will the ingress of liquid carbonic acid in the long tube; it is situated between the condenser of carbonic acid and this long tube. When this screw valve is closed, and the two pumps draw the vapor from the liquid carbonic acid contained in the tube four meters long, the greatest possible lowering of temperature is produced; the carbonic acid solidifies and descends to about $-140°$. The subtraction of heat is maintained by the working of the pumps, the cylinders of which take out three liters per stroke, and the speed is 100 revolutions a minute.

Both the sulphurous acid tube and the carbonic acid tube are covered with a casing of wood and non-conducting stuff to intercept radiation.

In the interior of the carbonic acid tube, B, passes a fourth tube, A, intended for the compression of oxygen; it is five meters long and fourteen millimeters in external diameter. Its internal diameter is four millimeters. This long tube is consequently immersed in solid carbonic acid, and its whole surface is brought to the lowest obtainable temperature. These two long tubes are connected by the ends of the carbonic acid tube, consequently the small tube extends about one meter beyond the other. I have curved this portion downward and given the two long tubes a slightly inclined position, but still very near the horizontal, as I have shown in the accompanying drawing.

The small central tube is curved at A, and screws into the neck of a large howitzer shell, C, the sides of which are thirty-five millimeters thick; the height is twenty-eight centimeters, and the diameter seventeen centimeters.

This shell contains 700 grams of chlorate of potash and 256 grams of chloride of potassium mixed together, fused, then broken up, and introduced into the shell perfectly dry. When the double circulation of the sulphurous and carbonic acids has lowered the



DESCRIPTIONS OF THE DRAWINGS.

A. A tube, 5 meters long, 14 millimeters external diameter, and 4 millimeters internal diameter, in which the oxygen condenses. It is furnished with a screw-tap, *r*, from which the liquid oxygen jets out. A pressure-gauge, M, measures the pressure up to 800 atmospheres.

B. A tube, 4 meters long, in which is solid carbonic acid. The stock of carbonic acid is contained in a gasometer, G, of 1 cubic meter capacity. A three-way tap, H, puts it when desired into communication with the apparatus.

C. A howitzer shell, containing 700 grams of chlorate of potash mixed with chloride of potassium. It is heated with gas.

P_1, P_2. Double-action exhaustion and force pumps, drawing carbonic acid from the tube B or the gasometer G, according to the position of the tap H.

S. A tube, 60 millimeters diameter and 1·1 meter long, in which is condensed the liquid carbonic acid compressed by the pumps. This liquefied gas returns by the small tube *t* to the tube B.

R. A tube, 125 millimeters in diameter and 1·1 meters long, containing liquid sulphurous acid.

P_3, P_4. Double-action exhaustion and force pumps, exhausting sulphurous acid gas from the tube R.

Q. A tubular condenser of sulphurous acid compressed by the pumps. This body, when liquefied, returns by the small tube *f* to the tube R. The cold water for condensing the sulphurous acid passes through the apertures E E.

a. Entry for liquid carbonic acid.

b. Exit for the vaporised carbonic acid caused by the suction of the pumps.

temperature to the required degree, I heat the shell over a series of gas-burners. The decomposition of the chlorate of potash takes place at first gradually, then rather suddenly towards the end of the operation. A pressure-gauge, M, at the extremity of a long tube, lets me constantly observe the pressure and the progress of the reaction. This gauge is graduated to 800 atmospheres, and was made for me expressly by Bourdon, of Paris.

When the reaction is terminated the pressure exceeds 500 atmospheres; but it almost immediately sinks a little, and stops at 320 atmospheres. If at this moment I open the screw-tap, *r*, which terminates the tube, a jet of liquid is distinctly seen to spirt out with extreme violence. I close the tap, and in the course of a few moments a second jet—less abundant, however—can be obtained.

Pieces of charcoal, slightly incandescent, put in this jet inflame spontaneously with inconceivable violence. I have not yet succeeded in collecting the liquid, on account of the considerable projectile force with which it escapes, but I am trying to arrange a pipette, previously cooled, which possibly may be able to retain a little of this liquid.

Yesterday I repeated this experiment before the majority of the members of our Physical Society, and we had three successive jets, well characterized. I cannot yet determine the minimum pressure necessary, for it is evident that I have a surplus pressure produced by the excess of gas accumulated in the shell, and which could not condense in the small space represented by the interior tube.

I hope to utilise a similar arrangement in attempting the condensation of hydrogen and nitrogen, and I am especially occupied with the possibility of maintaining low temperatures very easily, thanks to four large industrial pumps which I have at my disposal, worked by a steam-engine.

Geneva, December 25, 1877.

Since receiving the above we have been favored with further particulars of an experiment which was performed for the fourth time on Thursday, December 27th, in the presence of ten scientific men—among others, Professor Hagenbach, of Bâle, who came expressly to assist at this important experiment.

At 10 o'clock in the evening the manometer, which had risen to 560 atmospheres, sank in a few minutes to 505, and remained stationary at this figure for more than half-an-hour, showing by this diminution in the pressure that part of the gas had assumed the liquid form under the influence of the 140 degrees of cold to which it was exposed. The tap closing the orifice of the tube was then opened, and a jet of oxygen spirted out with extraordinary violence.

A ray of electric light being thrown on the escaping jet showed that it was chiefly composed of two parts;—one central, and some centimeters long, the whiteness of which showed that the element was liquid, or even solid; the other exterior, the blue tint of

which indicated the presence of oxygen compressed and frozen in the gaseous state.

The success of this remarkable and conclusive experiment called forth the applause of all present.

We understand that Messrs. Pictet & Co., of 22, Rue de Grammont, Paris, are fitting up apparatus with the intention of having these experiments repeated at their Freezing-Machine Works, at Clichy, in Paris.— *Chemical News, Jan.* 4.

2. *The Liquefaction of Oxygen, Nitrogen and Hydrogen.* Experiments of M. Cailletet. (From Nature, of January 3d.)— . . . It appears that as early as December 2, M. Cailletet had succeeded in liquefying oxygen and carbonic oxide at a pressure of 300 atmospheres and at a temperature of −29° C. This result was not communicated to the Academy at once, but was consigned to a sealed packet on account of M. Cailletet being then a candidate for a seat in the Section of Mineralogy. Hence, then, the question of priority (between M. Cailletet and M. Pictet) has been raised, but it is certain that in the future the work will be credited to both, on the ground that the researches of each were absolutely independent, both pursuing the same object, creating methods and instruments of great complexity. . . .

The methods employed by MM. Pictet and Cailletet are quite distinct and are the result of many years' preparatory study, as testified by M. H. St. Claire Deville and M. Regnault. It is difficult to know which to admire most, the scientific perfection of Pictet's method or the wonderful simplicity of Cailletet's. It is quite certain that the one employed by the latter will find frequent use in future experiments. M. Cailletet's apparatus consists essentially of a massive steel cylinder with two openings; through one hydraulic pressure is communicated. A small tube passes through the other, the sides of which are strong enough to withstand a pressure of several hundred atmospheres, and which can be inclosed in a freezing mixture. It opens within the cylinder into a second smaller cylinder serving as a reservoir for the gas to be compressed. The remainder of the space in the large cylinder is occupied by mercury. M. Cailletet's process consists in compressing a gas into the small tube, and then by suddenly placing it in communication with the outer air, producing such a degree of cold by the sudden distention of the confined gas that a large portion of it is condensed, a process perfectly analogous to that used to prepare solid carbonic acid by the rapid evaporation of the liquefied gas.

In M. Cailletet's experiment with oxygen it was brought to a temperature of −29° C. by the employment of sulphurous acid and a pressure of 300 atmospheres; the gas was still a gas. But when allowed to expand suddenly, which, according to Poisson's formula, brings it down to 200° below its starting point, a cloud was at once formed. The same result has since been obtained without the employment of sulphurous acid, by giving the gas time to cool after compression. M. Chailletet has not yet obtained, at all events, so far as we yet know, oxygen in a liquid form, as

M. Pictet has done; on being separated from its enormous pressure it has merely put on the appearance of a cloud. . . .

M. Cailletet first introduced pure nitrogen gas into the apparatus. Under a pressure of 200 atmospheres the tube was opened, and a number of drops of liquid nitrogen were formed. Hydrogen was next experimented with, and this, the lightest and most difficult of all gases, was reduced to the form of a mist at 280 atmospheres. The degree of cold attained by the sudden release of these compressed gases is scarcely conceivable. The physicists present at the experiment estimated it at −300° C.

Although oxygen and nitrogen had both been liquefied, it was deemed of interest to carry out the process with air, and the apparatus was filled with the latter, carefully dried and freed from carbonic acid. The experiment yielded the same result. On opening the tube a stream of *liquid air* issued from it resembling the fine jets forced from our modern perfume bottles.

These more recent results are all the more surprising as, at an earlier stage, hydrogen, at a pressure of 300 atmospheres, has shown no signs of giving way.

3. *On the Liquefaction of Acetylene, Ethyl hydride, Nitrogen dioxide and probably Marsh gas.*—Cailletet, studying the compressibility of acetylene, observed a marked departure from the law of Mariotte in its behavior, and, pushing the condensation still further, succeeded in liquefying it. The apparatus consisted of a hollow steel cylinder, thick enough to resist a pressure of several hundred atmospheres, having screwed into it at top a bronze collar carrying a thick glass reservoir closed above, and continued downward by a larger tube, open at bottom. This glass tube contains the gas to be liquefied, its lower end dipping below the surface of mercury in the steel cylinder. Hydraulic pressure forces the mercury up into the tube, compressing the gas and finally liquefying it in the narrow portion at top. In compressing acetylene, the temperature being +18°, numerous droplets are seen to form and run down the walls of the tube, under a pressure of eighty-three atmospheres. Reducing the pressure gradually, the liquid returns suddenly to gas filling the tube for an instant with a thick fog. Liquid acetylene is colorless and extremely mobile; it appears to be highly refractive, and is lighter than water in which it is soluble in all proportions. It dissolves paraffin and fats. Cooled to zero in presence of water and linseed oil, it forms a white compound like snow, which decomposes on heating or lowering the pressure. The tension of the acetylene vapor is as follows: at +1°, 48 atmospheres; at 2·5, 50; at 10, 63; at 18°, 83; at 25°, 94; at 31°, 103. Comparing the tensions of acetylene, ethylene, and ethylene hydride (C_2H_6) which contain in equal volumes, equal weights of carbon united to increasing quantities of hydrogen in the ratio 1 : 2 : 3, the author finds the tension of acetelyne at 1°, as above, to be forty-four atmospheres; that of ethylene at 0° according to Faraday, being forty-six atmospheres, and that of ethylene hydride—now liquefied for

the first time by Cailletet—at 4° being forty-six atmospheres, its liquefaction taking place at a pressure a little less than that of acetylene.

In a subsequent paper, CAILLETET announces the liquefaction of nitrogen dioxide, by a pressure of 104 atmospheres at −11°. At 8°, the dioxide is still gaseous under a pressure of 270 atmospheres. Marsh gas, compressed by 180 atmospheres at 7°, shows, when the pressure is suddenly diminished, a thick fog like that given by carbon dioxide under similar circumstances; whence the author hopes to effect its liquefaction.—*C. R.*, lxxxv, 851, 1016, Nov. 1877. G. F. B.

4. *On Flame-temperatures.*—ROSSETTI has made a series of experiments to determine the actual temperature of a flame by direct measurement. For this purpose he used a thermo-pile consisting of a pair of wires 0·3 millimeter in diameter, and forty centimeters long, one of platinum the other of iron, both enclosed in porcelain tubes, the ends, which projected about two millimeters, being united and then covered with kaolin sintered together. The free ends of the two wires were soldered to large copper wires, which ran to the galvanometer. The graduation of the apparatus was effected by means of thermometers for low temperatures, and for high ones, not exceeding, however, 825°, by means of a calorimeter. Since the curve given by these determinations was almost a straight line, especially the latter portions of it, Rossetti believed that he might continue it beyond the experimental limit in the same direction without marked error. In the flame of an ordinary Bunsen burner, this flame being seventeen centimeters long, he found the highest temperature, 1350° C;, to be in the colorless mantle of the flame, the violet zone having a temperature of 1250°, and the inner blue flame not reaching 1200°. In the dark non-luminous cone in the center, seven centimeters long, the temperature one centimeter above the opening of the burner was only 250°; at two centimeters above, it became 400° and so remained up to a height of four or five centimeters, reaching 650° at the height of six centimeters. Mixtures of air and gas were then burned in a Bunsen burner closed below, and the temperature measured in the hottest part of the flame. For one volume of gas and two volumes of air, this temperature was 1260°; for one volume of gas and two and one-half of air, 1150°; and for one volume of gas and three of air, 1116°. With four volumes of air the mixture would no longer burn in a Bunsen burner; and burned from a bat-wing burner gave a maximum temperature of only 958°. Mixtures of gas and carbon dioxide, burned in a Bunsen burner as above, gave, for one volume gas and one and one-half CO_2, a temperature of 1000°; one volume gas and two of CO_2 gave 860°; and with three of CO_2 780°. With four volumes CO_2, the mixture burned only in contact with a flame.—*Gaz. Chim. Ital.*, vii, 422, Sept. 1877. G. F. B.

5. *On the principle of Maximum Work, as illustrated by the Spontaneous decomposition of Barium perhydrate.*—As a funda-

mental deduction from his thermochemical researches, BERTHELOT proved long ago the tendency of chemical systems toward that composition which corresponds to the maximum evolution of heat. He now notes an excellent illustration of this law in the case of barium perhydrate, which decomposes spontaneously, while barium peroxide is permanent. A specimen of BaO_2 prepared in 1874 contained 9·4 oxygen in excess, and in 1877, 9·2 of this oxygen; showing its permanence. The hydrate however, $BaO_2(H_2O)_7$, prepared pure and kept moist, gradually decomposes, gas bubbles of oxygen developing in the mass, generating a pressure in the vessel, and forming a crystalline mass of barium hydrate BaO, $(H_2O)_{10}$. This decomposition is even more rapid under water. A specimen prepared in 1874 and kept moist, had lost in 1877 a fifth of its oxygen; while another portion kept under water had lost nearly all. In explanation of these facts, Berthelot shows: (1) that barium dioxide absorbs heat in decomposing, $BaO_2=BaO+O$ absorbing 12·1 calories, and hence cannot decompose without the aid of foreign energy; (2) that the decomposition of barium dioxide or perhydrate into barium hydrate and free oxygen, on the contrary, evolves heat, $BaO_2+H_2O=Ba(OH)_2+O$ evolving 5·52 calories if the water be considered liquid, or 4·0 if solid; and hence, as Boussingault has observed, aqueous vapor can displace the oxygen, the heat evolved being 15·2 calories, since the two bodies are in the same physical state. In the same way $BaO_2(H_2O)_7+(H_2O)_3=BaO(H_2O)_{10}+O$, evolving 10·6 calories, or 6·4, as the water is liquid or solid. Hence barium perhydrate decomposes spontaneously and at the ordinary temperature. "Thus," says Berthelot, "the secret of spontaneous decomposition of barium perhydrate is not to be found in any symbolic considerations, drawn from a figurative arrangement of atoms; but is explained by very simple and very obvious principles, resulting from the regular action of molecular mechanics."—*Bull. Soc. Ch.*, II, xxviii, 502, Dec. 1877. G. F. B.

6. *On the Hydrocarbon called Idryl.*—GOLDSCHMIEDT has submitted to extended examination the mixture of hydrocarbons found in the quicksilver chambers of Idria, from which Bödecker obtained the hydrocarbon which he called idryl. The crude material was the alcoholic extract of the chamber deposit, and fused from 75° to 86°. By solution in alcohol, difficultly soluble flocks were observed which were filtered off and marked A. They fused at about 200°. From the filtrate, or from the more fusible portions of the crude material, only very small quantities of A were obtained. On distilling the raw product in a current of CO_2, a white substance was obtained fusing at 100°, which was crystallized from alcohol and by sublimation (B). The residues were dissolved in alcohol and mixed with a boiling saturated alcoholic solution of picric acid. Red crystalline precipitates were thus obtained, most abundantly from the portions of lower melting point. On concentration, additional picrate was obtained but lighter in color as it was more soluble. The portions having

nearly the same fusing point were united and recrystallized till this point was constant; eighty fractions being in this way condensed into three. The first (C) was in dark red flexible needles, fusing at 220°; the second (D) was in large bright red brittle needles, fusing at 185°; the third (E) was in gold-yellow fine delicate needles, which fused at 144°. On examining the fractions obtained A yielded a small quantity of a body insoluble in benzene and probably chrysene; the larger portion was anthracene. The fraction B was phenanthrene, the fraction C pyrene, and the fraction D a new hydrocarbon having the formula $C_{15}H_{10}$, which though not identical with Bödecker's substance—this being probably a mixture of pyrene and phenanthrene—the author proposes to call idryl. Further researches upon its constitution are in progress.—*Ber. Berl. Chem. Ges.*, x, 2022, Dec. 1877. G. F. B.

7. *On the Determination of Nitrogen in Nitroglycerin.*—Lauer and Ador have made a series of experiments to ascertain the best method of determining nitrogen in nitroglycerin. Solution of the nitroglycerin from a dynamite with ether, evaporation of the ether and solution in potassium hydrate in excess, and then determination of the nitrogen by Reichhardt's method, gave too low results. The dynamite was then shaken with water, the deposited nitroglycerin dissolved in alcohol, the alcoholic solution treated with potash and the nitrogen determined as above; this method also gave too low results. Finally 30 grams copper oxide were mixed with the nitroglycerin and burned by Dumas's method. The combustion went on quietly and yielded the theoretical quantity, 18·5 per cent.—*Ber. Berl. Chem. Ges.*, x, 1982, Dec. 1877. G. F. B.

8. *On Aromatic Hydantoins.*—By the direct union of cyanic acid and glycocoll, hydantoic acid is produced: $\begin{matrix}CH_2NH_2\\|\\COOH\end{matrix}+CONH=CO\begin{matrix}<NH_2\\NH.CH_2COOH\end{matrix}$. By loss of water, hydantoin is formed $CO\begin{matrix}<NH-CO\\NH-CH_2\end{matrix}$. Hydantoin and its homologues methyl-hydantoin, may also be formed by fusing together glycocoll or the corresponding homologues of it with urea: $\begin{matrix}CH_2NHC_2H_5\\|\\COOH\end{matrix}+CO\begin{matrix}<NH_2\\NH_2\end{matrix}=CO\begin{matrix}<N(C_2H_5)-CH_2\\NH\text{———}CO\end{matrix}+H_2O+NH_3$. Schwebel has succeeded in forming a phenyl-hydantoin by fusing together phenyl-glycocoll with urea: $CO\begin{matrix}<NH\text{———}CO\\N(C_6H_5)-CH_2\end{matrix}$. Phenyl-hydantoic acid was not obtained. When however, potassium cyanide, ammonium sulphate and phenyl-glycocoll in aqueous solution are allowed to stand for some days at 40° the filtrate after separation of the potassium sulphate by alcohol, yielded abundance of phenyl-hydantoin.—*Ber. Berl. Chem. Ges.*, x, 2045, Dec. 1877. G. F. B.

9. *On the Behavior of Benzoic acid in the Organism of Birds.*—JAFFE has taken up anew the question of the change which benzoic acid undergoes in the organism of birds, first investigated by Meissner and Shepard. He confirms the result of these latter chemists that no hippuric acid is formed, but that an acid is excreted which like hippuric acid is a paired benzoic acid. To this acid he gives the name *ornithuric* acid. He prepares it by extracting with alcohol the fresh excreta of hens fed on benzoic acid, evaporating the alcohol, extracting again with hot absolute alcohol, and evaporating. The strongly acid liquid is mixed with water, and repeatedly agitated with ether, to remove fatty impurities. The residue is treated with dilute sulphuric acid, and again agitated with more ether. The etherial solutions are concentrated and allowed to stand for some days in a cool place. The ornithuric acid separates in crystalline masses, having when pure the empirical formula $C_{10}H_{20}N_2O_4$. Boiled with hydrochloric acid it gives benzoic acid and a new base $C_5H_8(NH_2)_2O_2$, diamidovaleric acid.—*Ber. Berl. Chem. Ges.*, x, 1925, Nov. 1877. G. F. B.

9. *Le Sage's Theory of Gravitation;* by JAMES CROLL, LL.D., F.R.S.—Le Sage's Theory of Gravitation is at present exciting a good deal of attention among physicists. This is perhaps to a considerable extent due to the fact that some of the conditions arbitrarily assumed by Le Sage in his hypothesis, have been proved to follow as necessary consequences from the kinetic theory of gases.

A clear and able account of this theory of gravitation has been given by Mr. Preston in the Philosophical Magazine for September and November last. Mr. Preston has endeavored to answer all the objections which have been urged against the theory; but in one case at least he seems to me to have failed.* It is a necessary condition of Le Sage's theory, in order that gravity may be proportional to mass, that the total volume of the free spaces in a substance in the form of interstices between the molecules must be great compared with the total volume of matter contained in the molecules themselves. This condition of free interstices Mr. Preston considers to be satisfied by assuming the molecules to be small as compared with their mean distances.

Were we at liberty to make any assumptions we chose in reference to the smallness of the molecules of matter and their distance apart, we might be able to satisfy the conditions required as to mass. This, however, we are not at liberty to do, for modern physics has enabled us to determine, at least roughly, the size of the ultimate molecules of matter and also the distances between

* In an interesting article on Kinetic Theories of Gravitation by Mr. W. B. Taylor published in the Smithsonian Report for 1876, he lays down six fundamental characteristics of gravitation with which every theory, he says, must agree. Of these six requirements, Le Sage's theory he maintains satisfies but two, namely, (1) that the direction of gravity is radial toward the acting mass, and (2) that its intensity diminishes as the square of the distance. But some of Mr. Taylor's objections have already been met by Mr. Preston in his memoir. Besides one or two of Mr. Taylor's fundamental postulates seem doubtful.

them. This subject has recently been investigated by Sir William Thomson, who has given full details of his result in a remarkable paper in "Nature" vol. i, p. 551. Sir William says the diameter of the molecule cannot be less than $\frac{1}{500,000,000}$ of a centimeter. The number of molecules in a cubic centimeter of a liquid or a solid may, he says, be from 3×10^{24} to 3×10^{26}. This gives the distance from center to center of two consecutive molecules to be from $\frac{1}{140,000,000}$ to $\frac{1}{650,000,000}$ of a centimeter. Now, if we take the mean of these two values we have $\frac{1}{300,000,000}$ of a centimeter for the distance between the centers. The mean *spaces* between the molecules are therefore less than the diameter of the molecules themselves. Under this condition of things it must be absolutely impossible that a gravific particle even though it were *infinitely small* could penetrate to the extent of a thousandth part of a centimeter, into the interior of a body without having its motion stopped by coming into collision with a molecule. Le Sage's theory appears therefore to be utterly irreconcilable with Sir William's conclusions regarding the size of the material molecule. But even supposing we were to assume, what we are hardly warranted in doing, that the molecules are 10,000 times smaller and their distances 10,000 times greater than Sir William Thomson concludes, still this would not assist the theory. The gravific particles would then, no doubt, penetrate a little further into the interior of a body, but beyond a few feet or perhaps a few inches no particle could go. (Communicated by the Author.)—*Phil. Mag.*, Jan., 1878.

10. *On the Thermal Conductivity and Diathermancy of Air and Hydrogen.*—Dr. Henry Buff, Professor of Physics in the University of Giessen has undertaken the revision of the work of Magnus, Tyndall and others upon this subject. The apparatus used was similar to that of Magnus, which is essentially as follows: Upon a vertical cylinder of thin glass, 56 mm. in diameter and 160 mm. in height, there was fused another cylinder of the same diameter but of 100 mm. in height. These cylinders were separated by a thin glass plate. A cork closed the lower opening of the cylindrical vessel and through this passed the glass tubes provided with stop cocks, by means of which the vessel could be filled with any gas at any pressure. A thermometer was inserted through a tubulature about 50 mm. below the thin glass plate placed in a horizontal position. The upper vessel was filled with boiling water, which was kept in ebullition during the experiment by the introduction of steam. To guard against radiation the apparatus was placed in a beaker and this beaker in another filled with water of a constant temperature. Magnus found with this apparatus that the heat rays are partially absorbed by their passage through air. With hydrogen, however, he found that the temperature increased with the density, and even at ordinary atmospheric pressure it had greater diathermancy than a vacuum. He accordingly concluded that hydrogen was similar to the metals in regard to conducting power. Dr. Buff's

apparatus was similar to that of Magnus, with the exception that a brass cylinder was cemented upon the glass vessel; instead of the thin glass plate a polished metallic surface, constituting the bottom of the brass vessel, faced the enclosed thermometer, a double wall surrounded the cylinder filled with cotton wool to prevent too rapid cooling. The glass cylinder was 20 cm. high and 7·5 cm. in diameter—its lower edge was ground so as to fit air-tight upon the plate of an air pump. The thermometer in Magnus' apparatus was superseded by a thermo-electric one formed of flattened German silver and iron wires soldered together. This junction is placed very near the bottom of the brass cylinder and the leading wires were conducted through the plate of the air pump. The whole apparatus was surrounded by a wider glass cylinder which also fitted upon the plate of the air pump and the enclosed space could be filled with water. With this apparatus, Buff discusses the controversy between Magnus and Tyndall in regard to the diathermancy of moist air. Although his experiments are not completed, Buff finds that his results agree pretty closely with those of Magnus, and cannot believe with Tyndall that the thermal absorption of moist air is from twenty to forty times greater than dry air. He sums up his results as follows:—

(1.) "The thermal conductivity of hydrogen and of other gases is far too small to admit of its being proved by the method Magnus adopted. The assumption that the conductivity of hydrogen is similar to that of the metals, if by this statement anything more is meant than that hydrogen, like solid and liquid bodies, is capable of transmitting heat from molecule to molecule, is therefore not justified.

(2.) On the other hand, hydrogen possesses a diathermancy closely approaching that of a vacuum.

(3.) Dry air absorbs from fifty to sixty per cent of the rays of heat which it receives from a source heated to the boiling point of water.

(4.) The absorptive power of moist air surpasses that of dry air by a trifling percentage, but by no means to such a degree as hitherto had been assumed by several physicists.

(5.) Rock salt is not absolutely diathermanous for the so-called dark rays of heat; its thermal color rather resembles that of dry air."—*Phil. Mag.*, Dec. 1877, page 401. J. T.

11. *Spectrum of the Electric Spark in compressed gases.*—Cazin and Wüllner have been experimenting separately upon the subject. Cazin expresses his results as follows: The electric spark resembles an ordinary gas flame. In both sources of light there are, beside the peculiar vaporous particles which give line spectra, other solid and liquid particles which give a continuous spectrum. The admixture of the last which arises from the character of the electrodes and sides of the containing vessel increases with the pressure, so that finally the line spectrum upon the higher continuous spectrum disappears. In the so-called aureole the mate-

rial particles are wanting, and the aureole can be compared to the blue basis of the ordinary gas flame. Cazin enclosed nitrogen in a species of quicksilver pizometer; the electrodes were of platinum. Beginning with two atmospheres, the channelled spaces and all the nitrogen lines save six disappeared. At ten atmospheres only the lines $\lambda=567$ and $\lambda=500$, together with a very bright line $\lambda=424$, which first appeared under five atmospheres, and was attributed by Cazin to nitrogen, remained. These lines which were not perceptible at ordinary pressures, together with the always present sodium lines and a platinum line which appeared at fifteen atmospheres of pressure, remained up to forty atmospheres. These results needed to be confirmed by the check of photography, and Herr Cazin has already obtained interesting confirmations by this process. The above results were obtained by passing the spark through the vapor of hyponitrous acid at a pressure of thirty atmospheres. Herr Wüllner objects to the conclusions of Cazin that the continuous spectrum is due to the broadening of the bright lines. According to Wüllner this is only the case for the hydrogen and a portion of the oxygen spectrum. The phenomenon noticed by Cazin in the nitrogen spectrum was due to admixtures of carbon compounds, especially the carbonates. Wüllner has obtained a third order of spectra from nitrogen, and he attributes Cazin's results to the fact that by increasing the pressure he was able to pass from one spectrum to another.—*Beiblätter Physik and Chemie*, No. 11, p. 620. J. T.

II. Geology and Mineralogy.

1. *Silurian Plants;* by Leo Lesquereux. 12 pp. 8vo. From the Proceedings of the Amer. Phil. Soc., Oct. 19, 1877.—Mr. Lesquereux has here published, with a plate for illustration, his latest facts respecting the land plants of the Ohio Lower Silurian, first announced by him in this Journal for January, 1874 (p. 31). The recent report by Count Saporta of a discovery of a fern from the Silurian slates of Angers, France, probably of the age of the Cincinnati group, first brought out in 1876, gives additional importance and interest to the earlier discoveries in Ohio. Mr. Lesquereux calls the species he first described *Protostigma sigillaroides;* and adds now, from the same rocks—the Cincinnati group, near Cincinnati, *Sphenophyllum primævum* Lesqx., *Psilophytum gracillimum* Lesqx. (near Covington, Ky., opposite Cincinnati).

In this paper Mr. Lesquereux also describes a Fungus (*Rhizomorpha sigillariæ* Lesqx.), found in connection with a Sigillaria in cannel coal at Cannelton, Beaver Co., Kentucky.

2. *Modified drift in New Hampshire;* by Warren Upham. 176 pp. roy. 8vo. From the third volume of the final Report of the Geology of New Hampshire. Concord, N. H. 1877.—Mr. Upham's memoir treats of the drift deposits, more or less stratified, which occur along the river valleys, about the lakes, and on the sea shores of New Hampshire. It is the result of a careful

study of these Quaternary deposits, carried on while acting as assistant geologist in the survey of the State. The conclusions here brought out with regard to the long gravel deposits of the larger valleys, which had been called kames, or eskers, are given at length in the last volume of this Journal, in an article contributed by himself. All parts of the subject are worked up with thoroughness and the facts are given with full details in the volume. The memoir is well illustrated by maps and sections.

3. *Ueber das Krystallsystem und die Winkel des Glimmers;* von N. v. KOKSCHAROW.—The monograph of the eminent Russian mineralogist upon the crystallization of the micas covers some eighty-five pages quarto, including the results of a long series of accurate observations. The principal conclusions arrived at are as follows:—That all the micas, without exception, belong to the orthorhombic system, with monoclinic habit; that with them all the plane angle of the base (cleavage plane), and also that of the fundamental prism is exactly 120°, consequently when the acute edges are truncated the prism is geometrically a hexagonal one. The ratio of the axes is c (vertical) : $\breve{b}$: $\breve{a}$=2·84953 : 1·73205 : 1. The author gives also a list of the crystalline planes observed by him and others, and a discussion of the methods of twinning.—*Memoirs of the Imperial Academy of St. Petersburg*, May 7, 1877.

E. S. D.

4. *Die Glimmergruppe;* 1 *Theil, von G. Tschermak.*—The memoir by Prof. Tschermak upon the mica family, of which the first part has been received, has been long promised, and is of especial interest in view of the earlier monograph mentioned above. In the present portion of the memoir, the author gives in detail his crystallographical and optical examination of the different species of the mica family, reserving the discussion of their chemical relations to a second paper. The exact determination of their optical characters has enabled the author to prove that all the micas, although a variation in angle from the orthorhombic form may not be established, are nevertheless *monoclinic.*

The micas are divided into two groups; with the first the plane of the optic ages is perpendicular to the plane of symmetry and with the second is parallel to it; they are as follows:—

	I.	II.
Biotites:—	Anomite.	Meroxene, Lepidomelane.
Phlogopites:—		Phlogopite, Zinnwaldite.
Muscovites:—	Lepidolite.	
	Muscovite.	
	Paragonite.	
Margarites:—	Margarite.	

The name *Meroxene*, first introduced by Breithaupt, is employed by the author to include all the magnesia micas of Vesuvius, and also all other magnesia micas closely related to them and not falling into the other divisions. On the other hand the magnesia micas which fall into the second class as defined above are called *anomite* (Gr. *ανομέω*); in this class falls the mica of L. Baikal, and that of Greenwood Furnace, N. Y.—*Vienna Academy.*

E. S. D.

III. Botany and Zoology.

1. *The Hybridization of Lilies;* by Francis Parkman.—In No. 15 of the second volume of the *Bulletin of the Bussey Institution*, under the above title, Mr. Parkman gives a summary of his experiments, during ten or twelve years, in crossing Lilies. One of the earlier results, and that which the horticulturists count as the eminent one, was the production of that magnificent hybrid between *L. auratum* and *L. speciosum*, with flower resembling the former in fragrance and form and the most brilliant varieties of the latter in color, which was brought out in England under the name of *Lilium Parkmanni*. The interesting physiological point which Mr. Parkman here records is, that this striking novelty was wholly unique; that all the other seeds of the same parentage which germinated, over fifty in number, gave rise to plants which in the blossom showed no trace of the male parent, *L. auratum* but were exactly like the female parent, *L. speciosum*. That these plants were truly hybrids, notwithstanding, is well made out, 1, by the precautions taken against any possible access of own pollen; 2, by the scantiness of seed, most of which was abortive; 3, "such good seed as there was differed in appearance from the seed of the same lily fertilized by the pollen of its own species," which is smooth, while this was rough and wrinkled, and 4, the stems were mottled after the manner of the male parent.

It would naturally be thought that this slight but evident impression of the character of the male parent might be deepened by iteration. That was tried next year, when the flowers of several of these plants were fertilized with the pollen of *L. auratum* precisely as their female parent had been fertilized. The result was an extremely scanty crop of seed, "but there was enough to produce 8 or 10 young bulbs. Of these, when they bloomed, one bore a flower combining the features of both parents, but though large, it was far inferior to *L. Parkmanni* in form and color; the remaining flowers were not distinguishable from those of the pure *L. speciosum*." The article records the results of various similar attempts to hybridize other lilies. For instance, our *L. superbum* was pollenized with eight different old-world species. The result was, that capsules, apparently perfect, were abundantly produced; some of them contained nothing but chaff, others had a few imperfect seeds, still others gave a fair supply of good seed. From this seed several hundred young bulbs were produced. "But when these came into bloom, not a single flower of them all was in the least distinguishable from the pure *L. superbum*." Moreover, in this case (different from the other) "not one of the eight different male parents had imposed his features on his hybrid offspring. Not only in their flowers, but in their leaves, stems and bulbs, the young plants showed no variation from their maternal parent." The experiment proceeded one generation farther. "In the following year I set some of them apart from

the rest, and applied to them, as to their mother before them, the pollen of several species of lilies. This time the seeds were extremely scanty. A few, however, were produced; but the plants and flowers that resulted from them were, to all appearance, *L. superbum* pure and simple."

In trials of other species results intermediate between these two cases were obtained. For instance the pure white of the perianth of *L. longiflorum* came out unstained in the progeny raised by a crossing with *L. speciosum;* and the herbage was equally unaffected; but in that or the next generation "distinct evidence could be seen of the action of alien pollen" in the changed color of many of the anthers, and in the abortion of others. They also showed differences of habit among themselves, some being very tall and vigorous, and others compact and bushy, with a tendency to bloom in clusters; but these may have been mere seedling variations, with which the hybridization had nothing to do." Yet some of these marks correspond with known results of hybridization.

That offspring should partake unequally of the characters of the two parents is a matter of common observation. That in the genus *Lilium* the hybrid offspring should in forty instances out of fifty take almost all its traits from the female parent, as Mr. Parkman has shown, is very remarkable. That, in not a few instances, it should take them all, so far as can be seen—that the paternal influence should be represented by zero—is most extraordinary. If parthenogenesis in plants were more unequivocally demonstrated, so as to be placed in certain instances quite beyond doubt (which is hardly the case), then we should regard the supposition which Mr. Parkman mentions as having been suggested to him, viz: that in the case of *L. superbum* the embryo was developed without male influence, to be quite as likely as the alternative of the progeny's inheriting everything from the female and nothing from the male parent; in fact the two suppositions approximate to the same thing. We are supposing the total absence of male parent's characters, and also that the alternative of fertilization by chance pollen of the species is absolutely excluded. Of this there is very high probability, yet not entire certainty. One of Mr. Parkman's "reasons for believing that parthenogenesis had nothing to do with the cases in question," viz., that some of the lilies were young plants that never had bloomed before, has no application, but comes from a slight confusion of the idea of parthenogenesis with the effect in some animals of a previous male influence upon next succeeding progeny, which is quite a different thing.

The fact that more than one sort of hybrid may be generated between the same two species, copulated in the same way, must do away with the old mode of naming hybrids by a combination of the name of the two parents, that of the male preceding. The plan had the double advantage of indicating the origin of the cross, and of distinguishing hybrids from species in nomenclature; but in practice it proves insufficient. A. G.

2. *Thuret's Garden.*—Upon the death of M. Thuret and the sale of his place at Antibes, it was feared that his noble and unsurpassed collection of living plants, of special botanical interest, would be broken up. But it is now stated (Gard. Chron., Dec. 15) that, "thanks to the generosity of Madame Louise Fould, it will for the future be devoted to public uses in connection with the Jardin des Plantes at Paris." An adjunct of this sort in Mediterranean climate is just what the Jardin des Plantes needs; and this ensures not only the preservation, but the increase and the scientific usefulness of a very rich collection of warm-temperate plants of all countries, specially those of dry regions. A. G.

3. *Dr. Engelmann's new botanical Papers in the Transactions of the Acad. Sci. St. Louis*, vol. iii, Nov.–Dec., 1877.—The most important of these papers is an appendix to that on The Oaks of the United States, read in the spring of 1876, and published soon afterward. A full notice of it appeared in this Journal. The continuation, of as many pages (21), is equally worthy of particular notice; but it is more difficult to give an abstract of it, and the space is wanting. Certain corrections are made, an improved classified enumeration of our species is given; then follow "additional notes," a continuation of these, and finally more additional notes. We have not yet the last word; but thoroughly conscientious work of this kind is most valuable. Now that the Oaks are comparatively clear, we look to Dr. Engelmann for the elaboration of our *Coniferæ*. A good contribution is made in the second paper before us: "*The American Junipers of the section Sabina*, which fills ten pages with the discussion of our nine species, Mexican and West Indian being included. The third paper is a small one on The Flowering of *Agave Shawii*, with a plate illustrating floral details. It must suffice merely to announce these publications, which are indispensable to working botanists. A. G.

4. *A new range for two Orchids* is given by the Rev. Dr. WIBBE of Oswego, New York, who sends *Listera australis* and *Habenaria leucophlæa*, gathered by him in "Lily Marsh," nine miles east of Oswego. The first was not known north of the pine barrens of New Jersey, and is a southern plant. The second belongs to the district from central Ohio west, but Mr. Hankenson had already detected it in Wayne Co., in the western part of New York. Pastor Wibbe also sends a polymerous state of *Trillium erythrocarpum*. Something of the kind not rarely occurs in *Trillium*. This plant, which has been constant since discovered five years ago, has all the parts from leaves to carpels regularly increased (in the leaves apparently by chorisis) from three to nine, except that the stamens hardly keep up to double the number of the petals. A. G.

5. *Botanische Untersuchungen über Schimmelpilze.* Part III. *Basidiomycetes;* by Dr. OSCAR BREFELD. Leipsic, 1877. 4to. —In this volume of over two hundred pages Brefeld gives a detailed account of his cultures of different *Basidiomycetes*. The work is full of interesting facts and observations and is excel-

lently illustrated. Had it been shortened by one-half, it would have been easier reading, certainly for foreigners. As it is, it is too detailed to commend itself to the general reader. The greater part of the book is devoted to species of *Coprinus*, especially *C. stercorarius*. The spores of this species which were sown in decoctions of horse dung germinated readily and produced first a mycelium and then a pileus and spores. In many cases a sclerotium was first produced from which grew later the pileus. In some species of *Coprinus*, but not in all, conidia (stäbchen) were produced. The object of Brefeld's cultures was to ascertain the existence of male and female organs in the Basidiomycetes. It had been stated by Rees and Van Tieghem that the "stäbchen" were male organs. Van Tieghem afterwards, however, changed his mind and concluded that they were conidia. Brefeld denies entirely that they are male organs and maintains that the pileus and spores of Coprinus are produced directly from the mycelium by a vegetative process without the intervention of any sexual organs. He arrives at this conclusion by the following process. He has examined the mycelium, the sclerotium, and all parts of the fruit-bearing body and finds no trace of sexual organs. But as the plants examined were complex, and it was possible that he had overlooked something, he goes farther and makes slices of the sclerotium, the stipe, and the pileus and places them in conditions favorable to farther growth. Now, if the fruit-bearing body is produced by a sexual action which takes place in the sclerotium, when the sclerotium is cut into several pieces only those pieces which contain the sexual organs can produce fruit-bearing bodies.

The same is true of cutting the stipe and pileus. Brefeld found that, however he sectioned the organs mentioned, the hyphæ of the cut surface grew up and formed one or more new fruit-bodies. Hence there can be no sexual organs either in the sclerotium, stipe or pileus. The mycelium and the hymenium in *Coprinus* are comparatively simple and it is impossible to recognize in them any sexual organs according to Brefeld. The latter part of the book is devoted to a consideration of some other *Basidiomycetes*, especially *Amanita* and *Agaricus melleus*. The last named species which is several inches high was raised by Brefeld from the spore in his cultures and he confirms the view of Hartig that the so-called *Rhizomorpha subcorticalis* is only the sclerotioid state of this fungus. Brefeld considers that *Rhizomorpha subterranea* is only an underground form of *R. subcorticalis*. In concluding, he gives some general views with regard to the Basidiomycetes and a table showing the relationship and descent of the different groups. Some of his views are novel, as, for instance, that the teleutospore of the *Uredinei* is of the nature of a resting-spore and that the promycelium and sporidia represent the basidium and spores of the *Hymenomycetes* as shown in the simplest form in some of the *Tremellini*. In this work we see about the first attempt to refer different species of fungi to a hypothetical type which was the common ancestor. This method which has been used with such advantage by zoologists is not

likely to answer as well in fungi of which almost no definite fossil remains exist. W. G. F.

5. *Beiträge zur Entwickelungsgeschichte der Flechten.* Part II. *Ueber die Bedeutung der Hymenialgonidien;* by Dr. E. Stahl. Leipsic, 1877. 8°.—An admirable essay, excellently planned, and beautifully written. In the first part of this work which has already been noticed in the Journal, Stahl gave an account of the sexual organs of lichens. In the present, he considers the signification of the hymenial gonidia of course in its bearings on the Schwendener theory that lichens are Ascomycetes parasitic on algæ. The species studied were *Endocarpon pusillum*, *Thelidium minutulum*, and *Polyblastia rugulosa*. Stahl considers that the hymenial gonidia are derived from the thalline gonidia and present a different aspect simply from their different surroundings. When the spores are discharged some of the hymenial gonidia are always discharged with them. As the spores germinate, their hyphæ fasten themselves upon the gonidia which then increase more rapidly than before. In from four to five months Stahl succeeded in raising new perithecia and spores by his culture of the spores of *Endocarpon* with the hymenial gonidia. In like manner, he raised perithecia and spores of *Thelidium* from culture of the spores with the hymenial gonidia. He went still farther. Some of the spores of *Endocarpon* together with the hymenial gonidia which had been discharged with them were placed in water in which the gonidia became diffused. Then spores of *Thelidium* were placed in the water and on germinating they attached themselves to the gonidia of *Endocarpon*. By such a proceeding Stahl was able to produce a thallus of *Thamnidium* with gonidia of *Endocarpon*. It will be seen that Stahl's experiments have a most important bearing in favor of the Schwendener theory. To Schwendener and Bornet we are indebted for the most accurate account of the anatomy of the thallus of lichens, but hitherto no one has been able to reproduce the fruit of a lichen by cultivating the spores with the gonidia. Bornet and Rees succeeded in making the hyphæ of certain *Collemaceæ* grow in *Nostocs*, but it was maintained by the opponents of the algo-fungal theory that the union of hyphæ with a *Nostoc* did not constitute a true *Collema* thallus because, as there was no fruit formed, it showed that the combination was not normal. By producing the lichen-fruit in his cultures, Stahl removes this objection. In constructing a thallus of one species by letting its spores germinate with the gonidia of another, he proves that the gonidia are neither produced by the hyphæ nor is there any essential connection between them except that of a parasitism. We notice the statement in the number of Just's Jahresbericht that has just appeared that the lichens will not be hereafter kept as a distinct group apart from algæ and fungi, but will appear as a subdivision of *Ascomycetes*. W. G. F.

7. *Acetabularia Mediterranea;* by A. de Bary and E. Strasburger. Ext. Bot. Zeit., Nov. 1877.—In this paper the development of *Acetabularia* is given partly by De Bary who cultivated

spores received from Antibes in his laboratory at Halle and partly by Strasburger who had studied the species in the Mediterranean. The anatomy and formation of the spores had been previously published by Woronin in the Annales des Sciences. To this De Bary adds an account of a peculiar process connected with the base of the plant. The spores produce a number of zoöspores which conjugate by twos or by larger numbers. Zoöspores from the same sporangium do not conjugate with one another but only with those coming from another sporangium. W. G. F.

8. *Entwickelungsgeschichte des Prothalliums von Gymnogramme leptophylla;* by Dr. KARL GOEBEL. Inaugural Dissertation. Strasbourg, 1877.—A detailed account of the growth of the prothallus of *Gymnogramme leptophylla.* The antheridia are like those of *Aneimia hirta.* The archegonia originate in a peculiar process, which projects from the under side of the prothallus. If the archegonia are not fertilized, the process grows into a new prothallus. The prothallus is also propagated by means of a kind of adventitious buds so that a single prothallus may live for several months and attain a considerable size. W. G. F.

9. *On a new Species of Parasitic Green Alga belonging to the genus Chlorochytrium of Cohn;* and *On a species of Rhizophydium parasitic on Species of Ectocarpus, with notes on the Fructification of the Ectocarpi;* by Prof. EDWARD PERCEVAL WRIGHT. Dublin Trans. R. I. Acad., vol. XXVI.—The species of *Chlorochytrium* was found by Prof. Wright on several plants, among others on *Calothrix confervicola*, where it was probably seen by Harvey and mistaken by him for the spores of that plant. The *Rhizophydium* as shown by Prof. Wright's plate seems to be the parasite which is very common on *Ectocarpi* of our own coast. At the end of the second article is a consideration of the different forms of sporangia found in British species of *Ectocarpus.* W. G. F.

10. *Transpiration in Plants.*—In the last number of this Journal, attention was called to a recent paper by Wiesner, showing that the rays of light which correspond to the absorption bands of the chlorophyll spectrum, are the most efficient in transpiration. Dehérain (in Ann. des Sci. Nat. for Sept., 1877) reviews Wiesner's communication and states that in 1869 he arrived, by a different method of experimenting, at an opposite result, namely, that yellow light is more active than blue in transpiration. Still claiming that his method is free from errors, he submits the following explanation of the contradiction: the rays which determine transpiration are the ones absorbed by chlorophyll, but they effect transpiration proportionately to their own convertible energy; it may therefore be supposed that a yellow ray, richer in calorific radiation, can act with greater energy, although only partly absorbed by chlorophyll, than can a blue ray, poor in calorific radiations, which is more completely absorbed. G. L. G.

11. *On Japanese Lingula and Shell Mounds.*—At a meeting of the Boston Society of Natural History, December 19, Professor EDWARD S. MORSE communicated some of the results of his work

in Japan. His main object in visiting Japan was to study more fully a group of animals upon which he has been at work for a long time—the Brachiopoda.

Accepting an appointment as professor of Zoology in the Imperial University of Tokio he established a Zoological station on the coast for the purpose of collecting material for the University Museum and for the training of Japanese assistants in the work.

His studies of Lingula have brought out many points new to science. The discovery of auditory capsules in the class of Brachiopods is one of the most important. These organs he determined in a species of Lingula and their position and general appearance recall the auditory capsules as figured by Claparède in certain tubicolous Annelids. He has also cleared up many of the obscure points in regard to the circulation, and is prepared to maintain the absence of anything like a pulsatory organ, the circulation being entirely due to ciliary action. Mr. Morse also described some of the habits of Lingula. While partially buried in the sand the anterior border of the pallial membranes contract in such a way as to leave three large oval openings, one in the center and one on each side. The bristles which are quite long in this region of the animal arrange themselves in such a way as to continue these openings into funnels and entangle the mucous which escapes from the animal; these funnels have firm walls. A continual current is seen passing down the side funnels and escaping by the central one.

They bury themselves very quickly in the sand, and the peduncle agglutinates a sand tube. They attach themselves by means of this tube to the bottom of dishes in which they are confined.

Mr. Morse exhibited living specimens of Lingula which he had brought from Japan in a small glass jar. The water had only been changed twice since August 20th, and yet no specimen had died. This illustrated more fully the vitality of Lingula than the experiments he had made on the North Carolina Lingula several years since.

A description was also given of an ancient shell-mound discovered by Professor Morse at Omori near Tokio, and photographs of many of the vessels exhumed were exhibited. The general aspects of the deposit were like those described by Steenstrup in Denmark, and by Wyman, Putnam and others on the United States coast. The implements were mostly horn. Only three rude stone implements were discovered. The pottery was remarkable in showing a great variety of ornamentation, though it was very rude in character and did not exhibit that finish seen in ancient Corean pottery which is found in the Empire. In the incised character of the markings it recalls the pottery of the east coast of the United States.

In the character of the raised knobs for handles on the edge of the vessels it shows the closest resemblance to pottery discovered by Professor Hartt in Brazil. Mr. Morse was not prepared to say whether it was early Aino or a race which preceded the Ainos and which the Ainos displaced in their occupation of the island from the north.

12. *The American Naturalist.*—The January number of this scientific monthly comes to us from Philadelphia, where this Journal is to be published hereafter by Messrs. McCalla & Stavely. Professor E. D. Cope is now associated with Professor Packard in the editorial management of the journal. It has always been successful in combining the scientific and popular in its articles, and has contributed greatly to science-education in the country as well as to the progress of science; and the Prospectus states that this will be still its aim. It is also announced that the department of birds will be edited by Dr. Coues, and that of microscopy by Dr. R. H. Ward; and that Professor O. T. Mason will continue his monthly summaries of anthropological news.

13. *Bulletin of the Nuttall Ornithological Club.* 48 pp. 8vo. Cambridge, Mass.—The third volume of this Quarterly Journal of Ornithology commenced with this year. The January number contains several valuable papers on birds and a very full selection of notes, besides extracts from the recent literature of the science. While thoroughly scientific, most of its articles have a popular interest, making it an attractive journal for the amateur as well as the man of science. It has for its frontispiece a beautiful colored plate of Baird's Bunting,—*Passerculus Bairdi* of Coues,—illustrating a paper by Dr. Elliot Coues, U. S. A. Professor J. A. Allen has a paper on *An inadequate "Theory of birds' nests,"* in which, after stating various facts, he expresses the following conclusion: "The most surprising thing about Mr. Wallace's 'theory of birds' nests' is its inadequacy and its irrelevancy to the facts it was proposed to explain; and in this respect it was scarcely excelled by any of the crude inventions into which the more ardent supporters of the theory of evolution by means of what has been termed 'natural selection' have been betrayed." The charge for the Bulletin is only two dollars per year.

15. *Nests of "the Gardener."*—In the Annali di Storia naturale del Museo Civico di Genova, the illustrious traveller and botanist, Prof. O. Beccari, describes the wonderful gallery or bower-constructions of the *Amblyornis inornata*, observed by himself in the Arfak Mountains. The huts and gardens, as built and laid out by this bird, which is called "the gardener," seem to surpass any production of intelligence and taste for the beautiful hitherto described and observed in birds of the Paradise family.—*Nature*, Dec. 6, p. 110.

IV. Astronomy.

1. *Meteors observed in Cambridge, Mass., November* 3, 1877.—Meteors were seen on this night at the following Cambridge times: $6^h\ 53^m$; $9^h\ 47^m$; $10^h\ 47^m$; $11^h\ 7^m$. They proceeded from the same radiant, which was near to *Mars*, and their directions were towards the S.W., all conformable to the above radiant.

C. H. B. AND L. H.

2. *Prize for the discovery of Comets.*—The Imperial Academy of Sciences at Vienna has resolved to continue, until further notice, the prizes, awarded since 1872, for the discovery of telescopic comets. The awarding of such a prize, consisting, according to the wish of the receiver, either in a gold medal or in its money

value of twenty Austrian ducats, is connected with the following conditions:

(1.) Prizes are awarded only for the first eight successful discoveries of each calendar year; for comets that, at the time of their discovery, were telescopic, i. e., invisible to the naked eye, that had not been seen before by any other observer, and the appearance of which could not have been predicted. The priority is to be decided by the epoch of the first position. (2.) The discovery is to be made known to the Imperial Academy of Sciences, immediately, and without waiting for further observations, by telegraph where practicable, otherwise by the earliest mail. The Academy will communicate the news without delay to several observatories. (3.) The first notice must contain as accurately as it is possible to the observer the position and motion of the comet, besides place and time of the discovery. This first notice is to be supplemented at the next occasion by later observations. (4.) If the comet should not have been verified by other observers, the prize will be awarded only when the observations of the discoverer are sufficient for determining the orbit. (5.) The prizes will be awarded in the general session held at the end of May of each year. If the first notice of the discovery arrives between the first March and the last May, the prize will be decided in the general May session of the Academy in the next year. (6.) Application for the prize is to be made within three months after the first news of the discovery has arrived at the Imperial Academy. Later applications will not be considered. (7.) The astronomers of the observatory of the University at Vienna will be judges, whether the conditions contained in arts. 1, 3 and 4 have been fulfilled.

3. *Index Catalogue of Books and Memoirs relating to Nebulæ and Clusters, &c.;* by EDWARD S. HOLDEN. Smithsonian Institution, Washington, 1877. 8°, pp. ix and 111.—About half of this valuable work of Professor Holden is devoted to the catalogue named in the title. The rest consists of: a list of books and memoirs relating to the nebula in Orion; a similar list of those relating to variable nebulæ; a list of drawings of nebulæ; and two indices to Sir Wm. Herschel's and Messier's Catalogues of Nebulæ and Clusters.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Telephone in England.*—Col. W. H. REYNOLDS has concluded a contract with the English Government by which the Post Office Department has adopted the Bell telephone as a part of its telegraphic system. In a recent telephonic experiment in connection with the cable 21¾ miles long, between Dover and Calais, there was not the slightest failure during a period of two hours. Though three other wires were busy at the same time, every word was heard through the telephone, and individual voices were distinguished. This important experiment was conducted by Mr. J. Bourdeaux, of the Submarine Telegraph Company. Some very successful experiments were made with the telephone on Saturday

night between Aberdeen and Inverness, a distance of 108 miles. Songs and choruses were distinctly transmitted, and conversation was carried on at times with marvellous distinctness, notwithstanding the weather was unfavorable. The experiments were made with Professor Bell's instruments. The Berlin correspondent of the Daily News states that a Berlin house is making a number of telephones for experimental use in the Russian army. The result is awaited with great curiosity in military circles. The Cologne Gazette denies that any telephone is in existence between Varzin and Bismarck's office at Berlin. Our contemporary says that the distance, 363 kilometers, is too large for using a telephone with any advantage.—*Nature*, Dec. 6, p. 109.

2. *A Manual of Heating and Ventilation in their practical application for the use of Engineers and Architects, etc.;* by F. SCHUMANN, C.E., U. S. Treasury Department. 89 pp. 8vo. New York, 1877. (D. Van Nostrand.)—This little book contains the formulas and data required by architects and engineers in putting to practice in the construction of buildings, the theoretical principles of heating and ventilation. The subject is systematically treated, and all the explanations are given that are required for an intelligent use of the formulas.

3. *Mesmerism, Spiritualism, etc., historically and scientifically considered;* being two Lectures delivered at the London Institution, with Preface and Appendix; by WM. B. CARPENTER, C.B., F.R.S. 158 pp. 12mo. New York, 1877. (D. Appleton & Co.) —The learned physiologist of London, Dr. Carpenter, has shown good sense, thorough knowledge and excellent judgment in these lectures.

4. *The Telephone;* an account of the phenomena of Electricity, Magnetism, and Sound, as involved in its action, with directions for making a speaking Telephone by Prof. A. E. Dolbear. 128 pp. 12mo. Boston, 1877 (Lee & Shepard).—This is a clear and interesting account of a subject which is exciting much interest at the present time.

5. AUGUSTE DE LA RIVE.—The number of the Bibliothèque Universelle (Archives des Sci. Phys. et Nat.) for September, 1877, has its 254 pages occupied with a biographical notice of the eminent Swiss Physicist, Auguste de la Rive, who died, at the age of seventy-two, on the 27th of November, 1873.

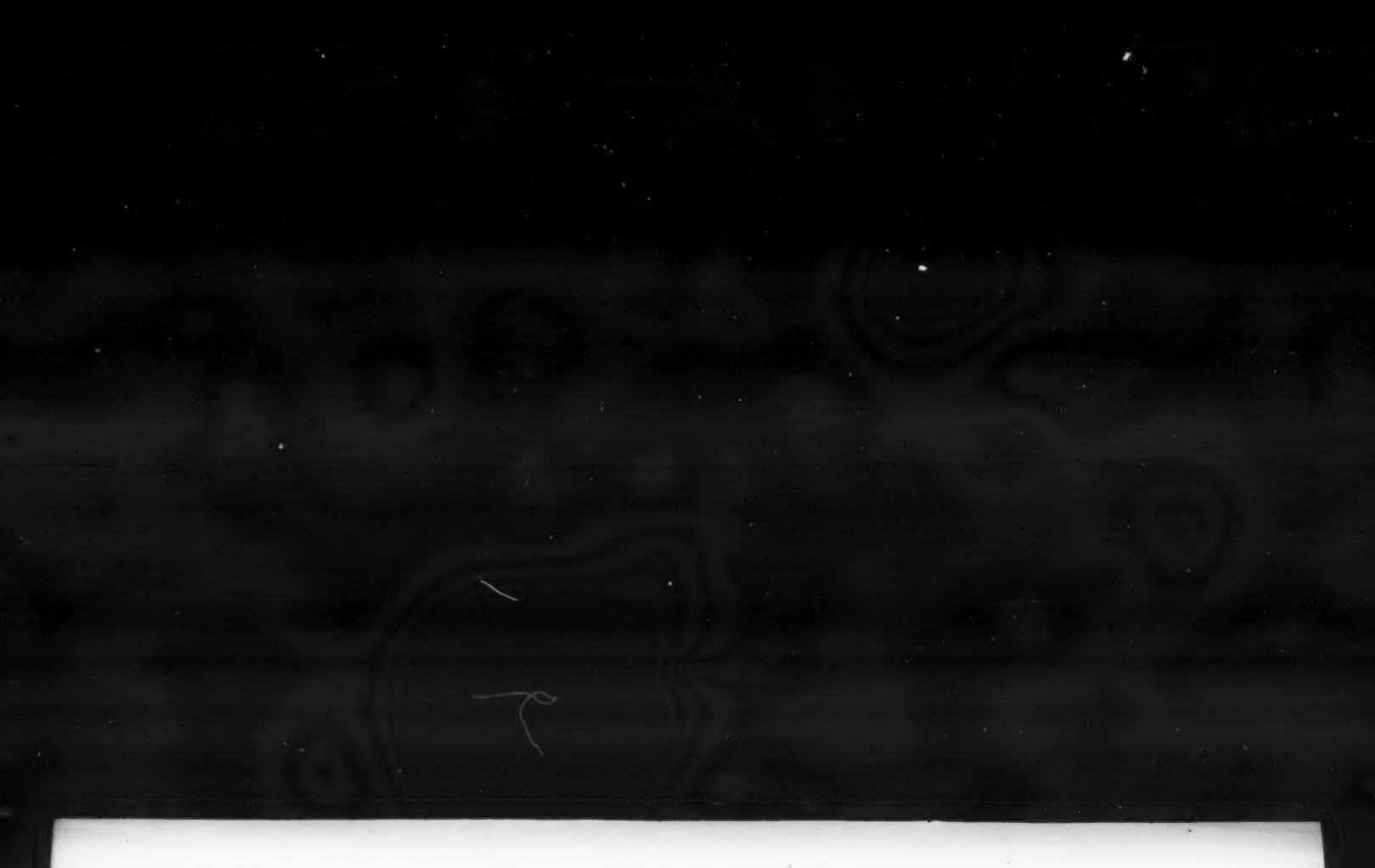
Proteus, or Unity in Nature, by Charles Bland Radcliffe, M.D. 214 pp. 8vo. London. (MacMillan & Co.)

The Physiology of Mind. By Henry Maudsley, M.D. 544 pp. 8vo. New York. (D. Appleton & Co.)

Meteorological Researches. By William Ferrel. Part I. On the Mechanics and the General Motions of the Atmosphere. 50 pp. 4to. With 6 charts. U. S. Coast Survey, C. P. Patterson, Superintendent. Washington. 1877. An important memoir discussing mathematically, from meteorological data with reference to the whole earth's surface.

OBITUARY.

RUHMKORFF died in Paris on the 20th of December at the age of seventy-four. He gave his first "Ruhmkorff Coil" to the world in 1851, and received for it from the French Exhibition in 1855 the first prize of 50,000 francs.



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Leipzig, im November 1877. **Joh. Ambr. Barth.**